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OUR PIONEERS
V
EDMUND GURNEY (1847-1888)
BY W. H. SALTER

LIKE Henry Sidgwick and Frederic Myers with whom he had been associated in psychical research for several years before he and they joined in founding our Society, Gurney was the son of an Anglican Clergyman, a classical scholar and a Fellow of Trinity, Cambridge. His academic success was a notable instance of the quickness with which he could master any subject that interested him, for 'he came up to Cambridge ill-prepared', as Myers says in his obituary notice (*Proc. V*), and gained his honours 'one may say, in the intervals of his practice on the piano'. Music was indeed a dominant passion with him all his life. When he was leaving Cambridge the Master of Trinity, the sharp-tongued Thompson, asked him what he was intending to take up, and on being told, Music, replied, 'You might as well say you are going to take up dancing!' Such was the contemporary estimate of those two arts at the University.

He continued to work at music for four years after he gained his Fellowship, but in 1875 regretfully concluded that he would never make any notable mark in it either as composer or executant. His interest in it remained unabated, as is shown by his book, *The Power of Sound* (1880), and by essays in *Tertium Quid* (1887), the second volume of which is mainly concerned with musical criticism. In the concluding essay, *The Psychology of Music* he writes :

I linger round a subject which has been to me, for as long as I can remember, both a central interest and—from the lack of natural facility and early training—a chronic torment. . . . Had the facility and the training been mine, and had I become a master of the art instead of being dragged at its chariot wheels, I should have troubled myself little, and others less, with speculations respecting it.

Early in the 'seventies' Gurney attended a course of lectures on physics which Lodge was giving in London, in order to obtain a scientific background for his musical studies. He was already devoting more and more of his energies to psychical research and to those provinces of psychology most closely connected with it. The friendship which grew up between lecturer and student resulted, as Lodge says, in his own first introduction to our subject.

Music apart he never attempted to follow any profession. Between 1877 and 1881 he did indeed study medicine, passing the second M.B.(Cantab) examination in 1880, and showing, as Myers says, 'unusual thoroughness, unusual penetration' in the scientific side of his training. But for the clinical side he showed no aptitude. His extreme sensitivity to suffering of every kind was a definite hindrance when it came to surgical dressing.

There was a strong legal tradition in the family, his grandfather having been a Baron of the Exchequer, and his uncle and guardian Recorder of London. It was therefore natural that he should pay some attention to legal studies, which however after a short experience he found a bore.

While success in any of the regular professions demands a high level of intelligence, it is often denied to intellects that, however acute, are of the speculative, inquisitive type, as Gurney's was. And for professional success no worse combination could be found than an intellect of that kind associated with a temperament expressing itself, as his did, now in moods of cheerful, almost boisterous humour, and now in melancholy pondering over the misery to which a large part of mankind seemed irretrievably condemned. It is desirable to emphasize the jocular side of his character, as it greatly impressed his Cambridge friends, among whom he was accounted one of the wittiest men of a witty circle.

The qualities which might have been a hindrance to him in law or medicine were of benefit when he decided to make psychological research his life work. There was no risk that this would bore his inquisitive intellect as conveyancing had done, and his sensitivity to the suffering of others, accentuated no doubt by his own persistent neuralgia and insomnia, spurred him on to follow an enquiry which he hoped might lead to some alleviation of their misery.

On Gurney's generation the impact of evolutionary theory was recent and violent, causing for many a sudden collapse of the traditional formulae of religion. Was there for them no possible alternative but some *ersatz* creed like Positivism, which treated with deplorable superficiality, as Gurney thought, 'two of the sharper ills of life,' physical suffering and bereavement? For the former

he looked forward to a substantial diminution. Of the latter he wrote (*Tertium Quid*, I. p. 32) that it admitted of 'no possible amelioration by diminution either of its extent or its bitterness'. 'Reason', Myers writes of him, 'had convinced him . . . that if there were a future life, the Universe *might* be good,' but only if there were.

Gurney had not himself any strong desire for a future life, and recognized that it was useless to speculate as to whether there was one until many aspects of personality which, like hypnotism, had been insufficiently explored by science, or, like paranormal faculties in general, entirely neglected by it, had been submitted to a thorough, systematic investigation. This might *possibly* provide a solution of 'the Controversy of Life' more firmly based on fact than the traditional formulae, more satisfying to human emotions than the substitutes for religion then being offered.

This is the dominant theme of the first volume of *Tertium Quid*, and it is in relation to it that the whole of Gurney's work in psychology and psychical research must be viewed. Convincing evidence of a future life eluded him, as it did Henry Sidgwick. On other problems of psychical research the book touches indirectly at several points, and one of the essays, entitled 'The Nature of Evidence in Matters Extraordinary' has a direct bearing on our subject. It is a devastating onslaught on the absurdities and inconsistencies of all who in the name of science refused to give psychical research even a hearing. It is also a lucid explanation, which has not lost any of its relevance in the seventy years or more since it was written, of the need for combining exact experiment with critical observation of spontaneous events.

Where phenomena cannot be commanded at will (as is the case in some of the more striking departments of our research . . .), the work of investigating them must consist, not in origination, but in the collection, sifting, and bringing into due light and order of experiments which Nature has from time to time given ready made. And the due estimation of these depends, in the broadest sense, on the due estimation of testimony,

not only on the 'acumen of the historical student', but also

on the general sagacity by which questions of probability and credibility, and disputes as to an accident, coincidence, and design, are decided in the matters of everyday life. (*Tertium Quid*, I, pp. 250, 251.)

No psychical researcher has equalled Gurney in experience of experiment and of observation in conjunction, and in this dual rôle his medical and legal training were of the greatest value.

The British pioneers of hypnotism, Esdaile, Elliotson and Braid,

were, as is well known, frustrated by the apathy, and sometimes the active hostility, of a profession that seemed more anxious to exploit their differences of opinion than to follow up their discoveries.

'Incredible as it may seem,' wrote Myers in his obituary,

in all the long interval from (say) 1855 till 1883—the date of publication of Edmund Gurney's first experiments—there was scarcely an experiment performed in England which added anything further to our knowledge: . . . when some of us in 1883-4 began to report from personal observation what was being done in France, and to add some experiments and reflections of our own, our papers were received with astonishment bordering on incredulity.

Lest it should be thought that Myers took too partial a view of the achievements of an intimate friend recently dead, I will quote from Dr T. W. Mitchell's paper, published fifty years after Gurney's death, 'The Contributions of Psychical Research to Psychotherapeutics,' (*Proc.* XLV, 175):

Gurney's experiments, therefore, were received with incredulity and few realized that he was laying the foundations on which the psychology of abnormal mental states during the next twenty years was to be based.

The theoretical implications of his research were, as Mitchell points out, mainly the concern of Myers.

More permanent interest attaches to Gurney's work as the main author of *Phantasms of the Living* (1886), Myers and Podmore being associated with him in its production. This is not the place to attempt a detailed appraisal either of the merits or the shortcomings of this, the earliest of the classics of psychical research.

It was a revolutionary thesis that apparitions of recognized persons, living or recently dead, which in some way conveyed information as to facts outside the percipient's normal knowledge or rational inference, were neither quasi-material wraiths nor products of a diseased imagination, but externalizations of telepathic impulses subconsciously received by sane and healthy persons. It depended on two propositions for which at the time there was little satisfactory evidence, first that telepathy was a real faculty, and secondly that it could present to a percipient who was not expecting anything of the kind a realistic visual or auditory likeness of the originator of the impulse.

Apart from the 'Phantasms', whose nature was under debate, the case for telepathy depended on a few experiments, particularly some where the subjects were the adolescent daughters of a clergy-

man named Creery. In some of the experiments with them one sister would act as agent and another as percipient, and suspicions arose as to their use of a code, which were ultimately verified beyond any doubt. But a definite discovery of the code was only made (*Proc. V*, 269) *after* the book had been published, and in it some of the results obtained under doubtful conditions were printed, though 'stress was never laid on any trials where a chance of collusion was afforded'. Even so, it was a serious tactical error to put so much stress on a series of experiments *any* of which were suspect. The authors were perhaps influenced by Barrett, who had conducted the first experiments with the sisters, and always strongly upheld the genuineness of their paranormal powers.

There has since been such an accumulation of experimental evidence for telepathy as to make the vagaries of the Creery sisters of little importance. The same cannot be said for the evidence of planned projection, so far at least as material reported in the Society's literature is concerned. *Phantasms* relates a few attempts by two anonymous agents (Vol. I, 102-10). There are a few in the Census report (1894), and very few, if any, since.

In Gurney's time therefore, the validity of his thesis, in which with some reservations his fellow authors concurred, depended mainly on the view to be taken of the spontaneous cases themselves, and though with our further knowledge of telepathy the situation has changed, the internal evidence of the cases is still material. If the reality of telepathy be presupposed, is this view a reasonable explanation of all the cases of the kind described above that are supported by good evidence? These last few words are the crux. It is possible to set up a standard of evidential perfection that could exclude all cases but a few, too few to support any generalization as to their nature and cause. A too inclusive standard on the other hand would defeat the theorist by confronting him with a hotch-potch of anomalous incidents irreducible to general principles. If however the cases printed in our *Proceedings* and *Journal* be taken as the norm, then it seems to me that the thesis of *Phantasms* covers satisfactorily so high a proportion of them as to raise doubts as to the validity of the rare cases which do not conform.

That is, I know well, a judgement with which several eminent psychical researchers would not agree. They would not however probably withhold their admiration from the patience with which the authors of the book sifted a large mass of material, much of it of poor quality, nor from their skill in analysing the most prevalent sources of error affecting this class of evidence, in the orderly arrangement of such material as satisfied their critical judgement, and generally in redeeming from 'anecdotalism' a wide range of

experiences long abandoned either to credulous hearsay or incredulous neglect.

On all his work as Honorary Secretary from 1883, and as investigator, Gurney lavished terrific energy. He 'lived on his nerves', as the saying is, tormented by neuralgia and insomnia, to relieve which his doctor gave him anaesthetics from time to time. He could still be cheerful and amusing. A friend with whom he dined one night wrote, 'I have rarely seen him in better health and spirits: and his conversation was brilliant.' The next night he took a fatal overdose, accidentally, according to the verdict at the inquest, and was found dead in bed. This was the first breach in the circle of friends who founded the Society and guided it during its early years.

CASE OF APPARENT AUDITORY HALLUCINATION

REPORTED BY ROSALIND HEYWOOD

WHATEVER may be its explanation, the following collective experience illustrates the extent to which minor details of evidence and unnoticed assumptions can affect the assessment of apparent spontaneous *psi*.

On being told of the case, my own immediate assumption was that as the phenomena were auditory and as the house where it took place was not far from the River Thames, with possible tidal effects on the structure, it was not worth further investigation, because the sounds reported could more reasonably be attributed to underground water than to ESP. It then occurred to me that this would be logical only if I believed ESP to be paranormal, which I do not. I look upon ESP as a natural phenomenon which I believe myself to have experienced and witnessed fairly often at first hand, whereas I have never experienced underground water as the cause of the kind of sounds quoted below. Moreover there were no cracks or signs of damp on the walls of the house. My second assumption was that because three women and two dogs heard the sounds these were more likely to be physical, in the ordinary sense of the word, but mistakenly interpreted. That this too was an assumption was brought home to me by the comment of

an experienced member of the S.P.R., that the case was much strengthened by the fact that three women and two dogs shared the experience. I therefore decided to make no more assumptions but to collect all the information I could about it. The reports by the persons concerned are as follows :

Report by Madame Kowalewska

55, Pimlico Road, London, S.W. 1

29 January, 1959

At approximately 10.45 on the morning of Thursday, 22nd January, 1959, I was working in my office with one assistant, Mrs Hagers. The premises consist of two rooms occupying the ground floor of my house (see diagram attached) to which there is but one front door. The front office, which originally was a retail shop and is now partitioned in two, is always cleaned on a Thursday morning. At the time stated I was working with Mrs Hagers in the small back office, and the cleaner, Mrs Borthwick, was washing the linoleum in the front office. The front door was opened and slammed shut and footsteps approached the curtain which hangs over the doorway leading into the offices. The footsteps paused, then continued to the door which leads to the rest of the house. This door was then opened and closed. When the front door slammed, Mrs Borthwick stood up and tapped on a glass fronted cabinet to draw my attention and my two dogs started to bark. I was not particularly surprised to hear Colonel Heywood (owner of the firm) come in, although he had told me earlier in the week not to expect him back from the continent until late on Friday evening, because he often finishes business abroad sooner than anticipated and just walks in, as he has a key. There was no doubt in my mind that it was Colonel Heywood because I recognized his footsteps. I have known Colonel Heywood for some twelve years and have worked in close co-operation with him for over three and a half years. I can tell what sort of a mood he is in by the way he walks and on this occasion he was in deep thought, his footsteps being slower and more measured than usual. When, after the hesitation behind the curtain, the footsteps continued to my private door I was puzzled, especially when the door was opened and closed. I got up and went out to see what he was doing, telling the dogs to be quiet. After a search inside and outside the house I found no-one so I went back to my desk. I then decided before starting work to go upstairs to the second floor to tell my husband what had happened. (He was ill in bed with influenza.) I remember grinning and saying to him : 'Frank has just walked in!' Having heard the story he urged me to telephone Mrs Heywood. After

some hesitation I decided not to do so because I thought I might alarm her, knowing that her husband would be flying home and remembering that I had once reported to her a premonition of an air disaster. When I returned to the office Mrs Hagers was very puzzled and wanted to discuss the matter, but I dismissed the incident to her as 'only the wind' because I did not want her to be alarmed. When I found that Colonel Heywood was not in the house I realized what had happened and I decided to ask him upon his return what he had been doing at the time. Apart from the footsteps, which were very clearly heard, the opening and closing of the private door was distinctive. When the handle is turned the whole lock squeaks and the noise of the tongue of the lock cannot be mistaken. The wind was blowing strongly, but at the time of the occurrence there was a curious calm, but immediately afterwards the volume of noise seemed to me to be greater than before. I forgot about the whole thing until this morning when Colonel Heywood came in, and then I thought of it again, probably because it was Thursday again. I asked him if he could remember what he had been doing last Thursday morning. He replied that he was in conference with our French licensees discussing the future of one of the articles we manufacture. I then asked if he had been thinking of Guardhook (meaning our office, although I did not say so) and he replied rather shortly that of course he had. I persisted and asked if he could recall just what had been going through his mind. He said that he had had a brilliant idea which would solve a problem which had cropped up here concerning some stiff pen mouldings. His brainwave was to dip the mouldings in boiling water to soften them. The only stiff mouldings of this type in this country are on these premises and he thought of getting the boiling water from my kitchen on the first floor. Colonel Heywood knows the layout of this house very well and has often gone upstairs to the kitchen to try out new ideas. When he had given me the above information I told him what had happened. His replies to my questions also solved the mystery as to why he had gone to my private door (which leads upstairs to the kitchen) and had not come into the office.

PEARL KOWALEWSKA

Report by Mrs Hagers

On Thursday 22nd, I was in the office with Miss Ainsley¹ and Mrs Borthwick was cleaning the front office. I think the time must have been roughly 10.45 a.m. as I start work at 10.0 and

¹ Mrs Kowalewska uses her maiden name, Ainsley, for business purposes.

although I had been working for some time, it was not yet time for coffee which is normally at 11 a.m. I heard the street door opened with a key and then slammed shut. I thought it was Col. Heywood as this was followed by three definite footsteps and I expected him to come in. After a pause the steps went on. The dogs were barking furiously and Miss Ainsley called to them to be quiet. I then heard the door to the flat opened. I knew which door it was because it always squeaks and the handle rattles. I then heard someone go up the stairs and I felt rather worried, because I knew Miss Ainsley's husband was ill in bed and by this time Mrs Borthwick had said there was no-one there, but I felt sure there was. Miss Ainsley also went to the main door and when she opened it she said it was very windy and it may have been the wind that rattled the door. I knew quite definitely this could not have been so, because the door was opened and closed exactly as Col. Heywood does it. He seems to have a particular way of flinging the door wide open and slamming it hard and then striding straight to the curtain which hangs at the entrance to the office. No-one else does this, as there are two small dogs in the office and we all open the door rather cautiously and after making sure that the dogs are not there, we shut the door carefully and usually try it again to make sure it is closed.

(signed) D. A. HAGERS

Mrs Borthwick's Report

29 January, 1959

I was washing the floor in Guardhook's main office when I heard a key in the door and somebody came in and closed the door. I stood up and tapped on the cupboard to draw Mrs K's attention, because I thought it was the boss coming in. The next thing I heard was the connection door between the office and flat being opened and closed. Mrs K came out of the small office and looked around to see where Col. Heywood was. There was nobody there.

(signed) K. BORTHWICK

Statement by Colonel Kowalewski

29 January, 1959

On January 22nd I was staying in bed with 'flu in our bedroom on the second floor of our house. During the morning my wife, who was in the office downstairs, came to see me and said, 'Frank just walked in!'

I was astonished, because I knew that Frank (Col. Heywood) was at this time in Paris on business. Then my wife explained to me what happened. They (my wife and two employees) were in the

office below, and suddenly heard somebody opening the entrance door, slamming it shut, cross the corridor, open the door to the staircase and go in. From the manner of entering and the steps they all had the same impression that it was Col. Heywood who came in and went inside, probably to the basement or to the kitchen upstairs. My wife told me that she went to investigate, but there was nobody.

We discussed this for some time and I advised my wife to ring Rosalind (Col. Heywood's wife) but my wife refused to do so, fearing that this might upset Rosalind, Frank being due to return from Paris by air.

(signed) JAN KOWALEWSKI

Statement by Colonel Heywood

27 February, 1959

On January 21st in Paris I saw my agents for the Poppet Glue Pen and they told me of the very serious situation in which they had been placed owing to a defect which had arisen in the course of manufacture in France.

We had experienced a similar trouble to a minor degree in England and that night when pondering over this I suddenly thought of a very simple cure for it.

On the morning of January 22nd I met my agents at their office at 9.45 and remained with them till 12 o'clock (11 o'clock English time), when I had to leave them hurriedly as I was late for my next appointment.

During the morning I explained to them my idea and, after considerable argument, persuaded them to try it. I was so sure it would be successful that I blamed myself bitterly for not having thought of it before as it would have saved Mrs Kowalewska a great deal of worry, and I visualized myself mentally telling her about it and going up to her kitchen with her to prepare the necessary boiling water.

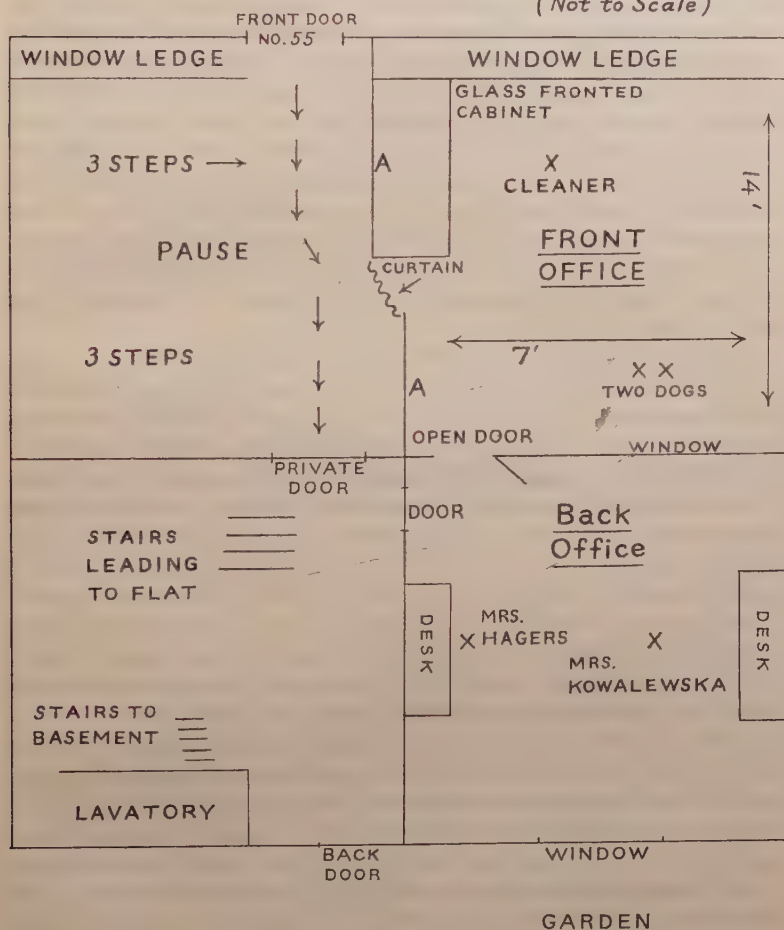
Further remarks by Rosalind Heywood

Two other persons besides the above have keys to 55 Pimlico Road. They have each written confirming that they did not go there on the morning of January 22nd. (*Letters examined by the Editor.*)

I have known Colonel and Mrs Kowalewski for over twelve years. During much of that time Colonel Kowalewski has worked for the Naval Intelligence branch of the Admiralty, and both he and his wife are fully aware of the importance of reporting her apparent *psi* experiences accurately and at once. She was also

well aware that she must give Colonel Heywood (my husband) no lead as to why she enquired about his activities on the Thursday morning. He told me that he thought her questions quite pointless. Her assistant, Mrs Hagers, and the cleaner, Mrs Borthwick both appear to be reasonable people.

NO.55, PIMLICO ROAD

*Sketch of Ground Floor
(Not to Scale)*

The partition (marked A on the diagram) between the front office and the passage does not reach the ceiling. On the window side of the entrance curtain it is made by a glass-fronted bookcase, seven feet three inches high, and on the other side by a thin wooden

partition the same height. The spot where Mrs Borthwick tapped on the bookcase, when she thought she heard Colonel Heywood come in, was less than five feet from the front door. I sat in the front office while Mrs Kowalewska opened and shut both doors for me, and the sound was loud, close and distinctive. Much as I wished to do so, I found it hard to imagine how, so close at hand, a series of varied and specific noises—key turning, door being opened and slammed, steps, more steps after a pause, second door opened by squeaky handle and further steps—could have been imitated by the gurglings of underground water or movements of the underground soil well enough to deceive three people. I also tried the latches, a yale lock and an ordinary door handle, and in both cases the tongue projected far enough to demand a considerable distortion of the door frames for its free passage. Again it is hard to see how underground water could have achieved this without other creaks and groans and disturbance of objects. It will be noticed that only Mrs Hagers, whose desk was against the wall beside the staircase, heard the footsteps go up the stairs. The others, who were further away, did not.

It might be suggested that the last person to come into the house had inadvertently left both doors unlatched, and that a gust of wind had crashed them open. But the wind would have been unlikely to shut them again at once, and the noise of the first door shutting was heard before the two series of steps and the pause which preceded the second door being opened. Also the three persons concerned say that they always take particular care to shut the front door, for fear that the little dogs should rush out on to a busy street.

My husband has a horror of letting down his clients and I had noticed before he went to Paris that he was very distressed about a difficulty which had arisen with his Poppet pens. He was correspondingly elated, when he came home on the Friday, at having thought of a cure in time to prevent some of the damage. On a number of occasions (see for example *Journal*, March 1945, 34, pp. 198–200) similar spells of emotional tension on his part have corresponded with impressions on mine of action he wanted taken, and Miss Phoebe Payne once said that she had ‘seen’ him at a place where he very much wanted to be, but where he was prevented from going. I record such an emotional ‘high tide’ on his part as I record that there was also a strong incoming tide on the Thames on the morning of the 22nd. I have ascertained from the Admiralty Tidal Department that it was high tide at Westminster about 12.15, which was an hour and a half *after* the sounds were heard. I have also ascertained from the Air Ministry that there was a

thunderstorm with heavy rain on the afternoon of January 20th, but slight rain only on the afternoon of the 21st.

One point mentioned by Mrs Kowalewska, that the sounds attributed to Colonel Heywood occurred during a curious silence, may be suggestive of temporary dissociation on her part. Some years ago she told me about a vision of some troops dressed in the uniform of the 1914 war and marching along Victoria Street towards the station. She said that while they were visible the sounds of the traffic around her ceased, but began again as the figures vanished. I did not report this incident as it could not be corroborated. When speaking of the present case she said to me that she found it impossible to explain that the sounds occurred 'in the silence' while at the same time the dogs were barking, because it did not make sense, but that was how it was.

MANOR HOUSE EXPERIMENT¹

REVIEW BY DENIS PARSONS, M.Sc.

It is a pleasure to be able to commend the first major experimental report to emanate from the College of Psychic Science since it succeeded in title the London Spiritualist Alliance. If such meticulous conduct and reporting of the College's investigations is to be the rule, the S.P.R. will have to look to its laurels. The report is signed by Brigadier R. C. Firebrace and by Sir John and Lady Swayne.

The aim of the experiment was to determine whether extra-sensory perception would be detected in conditions where a medium gave a sitting to an unknown sitter isolated in a separate room. The experimental sessions were held at a London doctor's flat on twenty-three evenings during the winter of 1957-8. The windows of all rooms in use for the experiment were closed and curtains were drawn across the windows during and between sittings. At all sittings the medium's utterances were recorded on tape; the duration of the sittings was 20 to 35 minutes.

For the first 7 sittings the unknown sitter was located in a room 50 feet away; for sittings 8 to 23 the sitter and medium were 12 feet apart in two rooms which were separated by a narrow lobby.

The possibility of collusion between medium and sitter was

¹ Report No. 1, 1957-8, published by The College of Psychic Science (1959, 104 pp. with plan).

considered—in particular the possibility that both might lean out of their respective windows and exchange messages. In the earlier sittings this possibility was excluded by the distance and by the physical obstruction of the view by a projecting chimney-stack. In the later sittings it was guarded against by the observer keeping constant watch on sitter and window through an open door.

The report of each sitting begins with a preamble headed : 'Time-table of events.' As an example of the precautions taken and of the careful reporting, here is the time-table of Sitting No. 20 :

1738 hrs. E.W.S. and J.G.S. arrived at the flat. E.W.S. entered. R.C.F. came out of the flat and joined J.G.S. in the car to await arrival of the medium.

1740 hrs. Mr H. Lazell arrived at the flat and entered.

1745 hrs. The medium arrived at the flat, was met at the entrance by R.C.F. and J.G.S. and conducted by R.C.F. to the examination room the door of which was then closed. J.G.S. locked the door from the treatment room into the lobby and kept the key : he also verified that the key-hole was covered.

1812 hrs. The sitter arrived at the flat and was conducted by R.C.F. to the treatment room the door of which into the hall was left open. E.W.S. and J.G.S. took up position as watchers in the kitchen and hall respectively.

1818 hrs. The sitting began.

1847 hrs. The sitting ended. The medium went to the kitchen where she sat with E.W.S. with door into the hall closed. The sitter, conducted by R.C.F. passed through the hall to the waiting room.

1907 hrs. The sitter was conducted through the hall by R.C.F. and left the flat.

Notes to the time-table of events

1. The Sitter was not aware of the identity of the medium and has signed Certificate A.

2. The experimenters declare :

- (a) that they are satisfied that at no time during the sitting was there any communication by sight or word between the medium and the sitter.
- (b) that they did not at any time before, during or after the sitting inform the medium of the identity of the sitter.

3. Mr H. Lazell, a member of the Council and Research Committee of the College, was present as observer during the sitting and was free to move about as he wished. [This ends the quotation].

Certificate A read as follows :

I certify that prior to 7th February 1958, when this series of experiments had ended, I was unaware of the identity of the mediums for any sittings at Manor House in which I acted as sitter. I have now been informed who the mediums were and I certify that I did not at any time prior to 7th February 1958, in conversation or otherwise, disclose to them that I was taking part in the experiment as a sitter.

Where Certificate A was inapplicable the sitter signed Certificate B :

I was aware of the identity of the mediums for the sittings at Manor House for which I acted as sitter but certify that I did not at any time prior to 7th February 1958, when this series of experiments had ended, disclose to them, in conversation or otherwise, that I was taking part in the experiment as a sitter.

Certificate A was signed by 10 sitters and Certificate B by 6 sitters.

The annotations of the sitters were compared by the experimenters with the statements of the medium and the correspondences were graded 'A', 'B', or 'C' according to the goodness of fit. "A" indicates evidence which by itself has only slight value but might be important as an addition to other evidence ; or evidence of which parts are good but diminished in value by other closely related parts which are incorrect. "B" indicates good evidence which, however, fails to convince beyond all reasonable doubt. "C" indicates evidence which convinces beyond all reasonable doubt.'

Unfortunately the results of the whole experiment were poor. This must have been most disappointing for the three investigators after their hard work although they declare themselves 'satisfied beyond reasonable doubt that on many occasions during this experiment some form of extra-sensory perception has been in operation'.

The mediums did not shrink from giving precise detail and unusual names such as Wilberforce, Conway, and Maby ; this bold entry into detail may have operated to reduce scores, but where hits were scored they might have been expected to be impressive. In fact 'Maby' was considered to be a possible case of precognition and 'Wilberforce' (Sir John Swayne informs me) has been recognised since the publication of the report. But out of 288 annotated sections we find only 35 grade A hits, 21 grade B, and 5 grade C (carrying conviction). The scoring and interpretation are very fair.

A partial weakness in the experimental design must now be mentioned. 'Most sitters checked the transcripts without knowing which referred to the sittings at which they had been the actual sitters.' Experience of S.P.R. members has been that it is excessively difficult for sitters not to mark generously the record which they believe to be that of their own sitting while undermarking records which they believe to pertain to other sitters. It is essential that *all* sitters should be asked to mark several transcripts, not knowing which is their own. The writers of the report under review have, however, presented a Table showing the gradings allotted by the experimenters to the goodness of fit of the medium's statements with annotations made by seven sitters who did *not* know which were their proper transcripts. Here it is :

Code letter of sitter	Annotations on own sitting			Annotations on other sittings		
	Grade A	Grade B	Grade C	Grade A	Grade B	Grade C
F	—	—	—	—	—	—
J	—	—	—	—	—	—
L	3	—	—	—	—	—
M	—	—	—	—	—	—
N	—	—	—	—	—	—
O	1	1	1	—	—	—
P	5	3	—	1	—	—

From the table it is seen that the annotations on those sittings which were proper to the sitters attracted 14 grade marks against 1 for reports on alien sittings. Although only one of the 14 is of top grade, the table is certainly suggestive. But even this evidence is somewhat weakened when we reflect that the *experimenters* knew whether they were grading the transcript pertaining to a given sitter or one foreign to him ; it would have been better to have entrusted the grading to independent judges. However, I must repeat that I formed a good impression of the fairness of the marking throughout.

The single grade C item in Table 2, which is regarded by the experimenters as a convincing case of precognition, runs as follows :

Medium : My sitter has had a bad finger or hand and I feel a bandage round the hand : I feel that a nail had to be removed or some minor operation had to be performed on the hand.

Comment : This appears to be a case of precognition. At the time of the sitting the sitter had nothing at all the matter with his hand. Ten

days later he wrote to report that he had that day been to St George's Hospital to have a small operation for the removal of a splinter from under the nail. The thumb was bandaged at the time of writing.

COMMENTS AND ADDITIONAL DATA ON SHEEP-GOAT CLASSIFICATION AND TARGETS

BY GERTRUDE R. SCHMEIDLER

At the outset of these remarks I should like to express my gratitude to Mr Scott and the Editor for having arranged that the response to Mr Scott's thoughtful review be published in the same issue (see p. 73). It was very good of them to permit both early clarification of issues which might otherwise seem incorrectly to represent disagreements between the reviewer and myself, and also prompt answers (insofar as I can give them) to his pointed and relevant questions.

Criticism 3

Mr Scott's statement of the problem of retrospective rejection or reclassification of subjects sent me back to my files, to tabulate the data on all records which had been omitted because it seemed impossible at the time to classify the subjects as either sheep or goats. None of these omitted records has been thrown away; and to the best of my knowledge Table 1 lists all of them. Subjects' comments and ESP scores are given. The reader who thinks that certain of the subjects should have been included in the book's totals as either sheep or goats can therefore recalculate those totals.

My procedural rule, followed without exception, was that subjects were required to respond to the questions from which 'sheep' or 'goat' classification was determined before they learned any of their ESP scores. In all cases where subjects were tested individually, and in some of the group sessions, subjects responded to the sheep-goat questions before they made any ESP responses. A few of these subjects (Table 1A) later reported a change in attitude, sometimes stating that seeing their scores had made them change their minds. It was not—and still is not—clear to me whether it would be better procedure to keep such a subject in his original classification or to list him as a sheep for certain of his runs and as a goat for the others. With full data listed below the

reader may choose for himself—and perhaps he will decide as I did that when comparatively few records are tied up in such a knotty question, it is more practical to cut them out than to try to disentangle them.

Other subjects omitted because of difficulty in classifying them as sheep or goats were two (tested individually) whom I had misunderstood and therefore tested inappropriately in the early days when I used different test conditions for sheep and goats (Table 1B), five whose responses were ambiguous or absent (Table 1C) and a large group to whom incorrect instructions had been given (Table 1D).

Mr Scott properly raises the question of whether my knowledge of the ESP scores could have affected either my classification of the subjects or my decision as to whether a response was ambiguous. It could not have done so in any of the individual tests, where the classification was made before the ESP responses. Nor could it have done so for 795 of the group records, where sheep-goat data were on separate sheets from ESP responses and were classified before I knew the ESP scores. (These include the two semesters which Dr McConnell reports examining in depth: those beginning September 1949 and February 1951.) For the remaining 362 group records, however, where subjects put the sheep-goat responses and the ESP responses on the same sheet of paper and also had their own (often inaccurate) scoring of ESP responses on this sheet, a more detailed answer to his question is required.

On re-examining the records with as critical an attitude as I could adopt, it seemed to me that 358 were not susceptible to biased interpretation. The great majority responded with only 'Sheep' or 'Goat'. A few added some response which was irrelevant (such as 'Sheep-baa baa') or which seemed only to confirm their choice (such as 'Goat—if I get any right it will be a miracle' or 'Sheep?' or 'Slightly sheepish'). I list in Table 2 the cases which in my opinion offer any possibility of misclassification. (The first of these, incidentally, I now think should have been omitted.)

Once a decision had been made to categorize a subject as a sheep or a goat or not to class him as either, this decision was not altered. Special examination of low scoring sheep and high scoring goats has modified my interpretation of what these responses imply about psychological dynamics, but it has never resulted in a change of the subject's classification.

Criticism 4

Mr Scott's question about target randomization and his ingenious hypothesis about systematic miscoding has brought about further

studies of the data. The first of these was generously performed by Mr Fraser Nicol and Dr Betty Nicol. They used for detailed analysis the twenty-four target decks of the final semester of the research, since in this semester the sheep mean was highest of all the group tests, and the difference between sheep mean and goat mean was greatest. Their results, summarized in Tables 3 and 4, do not seem incompatible with the hypothesis that the targets they examined were random.

A second analysis consisted of my tabulating the distribution of all target items for the group tests. The results astonished and dismayed me: they showed that without my having been aware of it, I had been using closed decks for a very large number of targets. This of course means that the probability figures previously cited need revision for the group series, because the theoretical standard deviation for closed decks is not 2.00. (It seems unlikely, however, that such revision will change the general trend of the data, or alter appreciably any of the probability values.)

To try to understand how these closed decks could have been used, I looked at the target sheets for clues; but the form in which they are retained in my files makes them unidentifiable. Then this possibility occurred to me. In 1946, when my reserve of targets was running low, some of us agreed that instead of hiring another assistant to make up new lists, it would be proper, economical and easy for me to use earlier targets prepared by a previous assistant for other quite separate projects. A colleague therefore gave me a large number of targets used in earlier (inconclusive) research. Since this colleague was a careful person and thoroughly familiar with my method of having targets prepared, I assumed that the lists were appropriate ones, added them to my supply with gratitude and used them along with the others in my reserve. A few of these sheets (which I did not use for formal tests) are still identifiable, and (I now find) consist of closed decks. It therefore seems likely to me that those targets which were prepared by an assistant whom I did not myself instruct were the source of all the closed decks.

A further difficulty in the tabulation of frequencies concerns 16 of the 419 targets. When divided according to separate sessions¹ it is clear that in most sessions there were no closed decks, and in other sessions all decks were closed. But in 10 sessions using 85 targets, all except one, two or three of the decks were closed. For these 16 irregular decks interspersed among the 69 closed ones, in 14 instances the symbol distribution was 4-5-5-5-6, in one

¹ Another error in the book must be noted. There were 48 separate administrations of the group tests, not 37 (as stated on page 42).

instance it was 3-4-5-6-7 and in one instance it was 3-4-4-7-7. Because of the context of closed decks, and because of the flat distributions, this looks to me as if, for 16 closed decks, the assistant had made errors in transcription. Subsequent tables list these 16 decks separately (except in summaries). The appearance of such presumptive errors forcibly raises the question which Mr Scott raised for other reasons : were the targets random?

The data on frequency of occurrence of each target item are given in Table 5. Differences from chance expectation are not significant, so the targets pass at least the first of their tests. There is no support for the neat hypothesis about systematic miscoding of zeroes. (New *ad hoc* hypotheses could of course be developed, arguing for example from the slight observed surplus of waves in the targets that the higher ESP scores of sheep than of goats might be due to the sheep but not the goats having a stimulus preference for waves. However in the two semesters which showed greatest mean difference between ESP scores of sheep and goats there was no surplus of waves, so it seems unlikely that this particular argument could be defended.)

In correspondence with me, Mr Scott suggested that I apply a further test : examination of the number of times a target item is repeated without different intervening items, making a sequence of AA, AAA, AAAA, etc. (This is considered a sensitive test of whether the targets were made up out of someone's head, for it is common experience that most people do not repeat items in sequence as often as would be expected by chance. Thus if the target lists had been invented or markedly modified by an assistant, we might anticipate that the number of sequences of repeated items would be substantially less than theoretical expectation.) The suggestion seemed a good one, and I acted on it. The data are summarized in Table 6.

The over-all number of sequences of two, three and four items is not significantly different from chance expectation for closed decks, open decks and total decks. It should be noted that if the sixteen doubtful decks are evaluated as open decks, there is a significantly small number of sequences of two items ($P < .01$), but if they are evaluated as closed decks the pattern is not significantly different from expectation ($P = .20-.10$). Analysis of sequence patterns for each symbol and colour gives satisfactory values except for a marked surplus of sequences of three crosses, and a corresponding deficit of sequences of two crosses followed by some other symbol. By and large, these irregularities do not seem to indicate that there was systematic miscoding, or that the lists were invented, or that there are radical departures from a random pattern.

A large—perhaps an infinite—number of tests of randomness could be applied, but none has been used on these targets except the ones reported above. It will be remembered that the basic plan of the research involved the precaution of repeated experimental series; our feeling was that the irregularities which each set of targets would surely show would not be likely to result *repeatedly* in the same pattern of group differences. Thus the question of systematic miscoding or inventing lists is relevant to our research design, and I am grateful to Mr Scott for raising it. However the question of randomness of targets (which we thought were drawn from random number tables) had seemed to me an irrelevant one. And even now it is difficult for me to see how the repeated differences in ESP scoring level between sheep and goats could be explained away by any target irregularities other than those produced by invention or systematic miscoding.

Further Questions and Comments

Mr Scott writes that it would be interesting to know why the ratio of sheep to goats varied widely from one term to the next. Probably a major factor was variation in the preceding class discussion of ESP. This sometimes consisted of little more than two or three minutes of remarks by myself; sometimes it lasted for an hour. In longer discussions the topics were determined by questions from members of the class, and it is my impression that when there was more talk of experimental data there were more sheep; when there was more talk of precognition, spontaneous cases, fraud or theory, there were more goats. Another factor might have been differences in general class morale, due to recently returned papers or examinations with preponderantly high or low grades. The subjects' reluctance to offend me by calling themselves goats, or their desire to show they were open-minded by calling themselves sheep, might have been influenced by subtle changes in my tone or my wording of instructions. But since no contemporary records were made of any of these points, the foregoing is speculative.

It seems to me that Mr Scott and I are in essential agreement about how to interpret any positive findings of differences in ESP scores between sheep and goats. Neither of us would take such findings to mean that disbelievers must, as a direct consequence of their disbelief, score fewer ESP hits than other subjects. We would agree, I think, that the subject's attitude toward the ESP situation (which is indicated only to a small extent by his answer to the sheep-goat question) is likely to be one factor influencing the number of hits. And perhaps he would agree with me further

that the experimenters' attitudes, feelings—and ESP potential—interrelating with subjects' attitudes, feelings and ESP potential are among the factors we need to know; we should be studying interrelations, not isolated items. My only disagreement with Mr Scott's comments on this issue seems like splitting hairs: I would argue that such relations underlie, rather than undermine, any sheep-goat finding.

TABLE I
SUBJECTS OMITTED FROM 'ESP AND PERSONALITY PATTERNS'
BECAUSE OF DIFFICULTY IN CLASSIFYING THEM AS EITHER
SHEEP OR GOATS

Initial classification	Individual or group session	Difficulty	Hits per run (25 calls)	Total hits	Total runs
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A. Subjects Whose Classification as Sheep or Goats Changed during the ESP Session

Goat ¹	Indiv.	Said at end of session that he had varied between thinking that ESP was nonsense and that it 'was true'	5 5 7 5 5 3 3 4 6 4 5 5 6 5 5 6 13 9 6 5 7 9 4 5 6 8 10 9 6 5 0 4 4 4 3 4 5 5 5 4 7 3 9 7 4 6 1 6 9 7	278	50
Goat	Indiv.	After sixth run became sheep	8 7 5 4 6 3 2 2 3	40	9
Goat	Group	Stated, 'Opinions changed to hopefulness after beginning and understanding purpose of test.'	3 2 7 6 4 4 4 4 4	38	9
Sheep	Group	After sixth run changed to goat	3 7 5 5 6 6 6 3 6	47	9
Sheep	Group	After third run changed to goat	4 3 5 3 6 7 4 4 3	39	9
Sheep	Group	By end of session had become a goat	2 3 3 4 6 5 7 6 4	40	9
Sheep	Group	At unknown time erased 'sheep' and wrote 'goat'	3 6 5 4 4 8 8 4 9	51	9
Sheep	Group	At unknown time wrote 'goat'	4 6 5 5 6 5 6 4 9	50	9
Sheep	Group	At unknown time wrote 'goat'	5 6 2 5 8 3 6 2 4	41	9
Sheep	Group	After fourth run changed to goat ²	5 7 4 4 8 5 3	36	7
Sheep	Group	After first run changed to goat ²	4 1 5 5 6 5 3	29	7
Sheep	Group	After first run changed to goat ²	5 5 6 6 7 5 2	36	7

B. Subjects Initially Misclassified, In a Series Where Sheep and Goats Were Tested Under Different Conditions

Sheep ¹	Indiv.	After third run said he was a goat except for telepathy	5 4 4	13	3
Sheep ¹	Indiv.	After ninth run said he had believed from the first that hits were due only to chance	7 6 5 2 3 5 5 5 7	45	9

TABLE I—continued.

C. Subjects Whose Responses Were Ambiguous

Goat ¹	Indiv.	Said at end of session (without having seen his scores) that he was sure he could guess better than the average person, though he was sure ESP was impossible	5 7 5 4 6 5 6 4 4 3 6 11 4 3 5 4 8 4 5 5 5 3 6 2 2 5 4 6 9 5 5 6 3 7 7 9 4 7 5 6 6 7 4 6 5 3 4 4	249	48
None	Group	Wrote only '?'	5 6 10 4 3 4 6 4 5	47	9
None	Group	No response	3 7 6 5 5 6	32	6
Sheep	Group	Added 'impossible'	2 2 5 3 3 4 6 6 4	35	9
Sheep	Group	Wrote lengthy comment expressing the idea that ESP was impossible	7 4 6 7 5 7 7 8 9	60	9

D. Subjects Whose Instructions Were Incorrect

Goats ²	Group	For one class, instructions were given incorrectly. Subjects understood that they were to classify themselves as goats unless they were certain that ESP could occur under the conditions of the experiment. The nineteen subjects listed here were all who called themselves goats. In the following class period most of these subjects reported that they would have called themselves sheep if they had been given the usual instructions. (Only four subjects called themselves sheep in this class.)	2 0 2 10 3 8 5 4 4 6 5 2 6 7 8 6 6 7 4 6 6 6 7 9 7 8 3 0 7 4 7 9 9 5 3 3 4 11 5 4 6 6 3 1 6 5 6 5 6 2 6 5 6 6 2 3 2 2 8 3 6 11 8 2 3 6 5 7 6 7 2 5 9 4 5 4 8 3 6 7 5 8 8 4 8 7 5 3 4 3 7 9 5 4 5 4 4 4 4 8 5 7 5 5 1 6 6 6 4 5 6 4 6 7 4 6 3 6 4 5 7 6 5 3 7 3 3 5 2 5 5 5 5 4 6 4 2 8 9 5 7 3 6 4 7 4 5 6 1 6 7 5 4 8 4 6 6 6 6 5 5 5 5 5 4 2 4 4 6 5 3	38 53 53 53 46 47 45 43 51 50 46 49 45 46 46 48 45 51 38	9 9 9 9 9 9 9 9 9 9 9 9 9 9 9 9 9 9 9
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¹ Omission of data reported in *J. Amer. Soc. psych. Res.*, 1943, 37, pp. 216-7.² It is embarrassing to report that only now do I realize that instead of omitting all scores for these subjects, I omitted only the initial 'sheep' scores. These three subjects were listed among the goats, and their scores for the latter runs (8, 5, 3; 1, 5, 5, 6, 5, 3; 5, 5, 6, 7, 5, 2) were included among the goat scores in all my tabulations.³ Omission of data reported in *J. Amer. Soc. psych. Res.*, 1947, 41, p. 41.

If repeated questioning of the subject about his attitudes persuades him to a concern with his own introspections rather than with the ESP task, the device of repeated questions could be an unfortunate one. A task-oriented question like the one Mr Scott proposes ('How many successes do you expect to get?') meets this objection, and would indeed be interesting to ask before each run. But it is well-known that level of aspiration shows marked individual differences in relation to achievement. It would therefore be important to run a control series in such research: to administer to each subject repeated non-ESP tasks, ascertain for these tasks the relation between his stated expectation and his success, and only then predict the relation between his stated expectation and his hits in ESP. The reason that I did not use this approach systematically was lack of time, not lack of interest.

In short, I should like to register strong agreement with the suggestion that the 'true' (personality) determinants of ESP scores may be only slightly related to sheep-goat attitudes. Let's keep looking for them!

TABLE 2

SUBJECTS WHOSE SHEEP-GOAT CATEGORY WAS DETERMINED AFTER THE EXPERIMENTER HAD SEEN THEIR OWN SCORING OF ESP HITS, AND WHOSE RESPONSES MIGHT BE CONSIDERED AMBIGUOUS (ACCORDING TO THE EXPERIMENTER'S JUDGMENT ON RE-EXAMINATION)

Category	Subjects' responses	Hits per run	Total hits	Total runs
Sheep	Sheep. Yes, but only for 3 runs	5 3 4 4 5 4 5 3 5	38	9
Sheep	There may be some relationship, but I doubt it. However, the results should prove interesting	6 9 10 4 5 3 3 9 5	54	9
Goat	Goat—for normal people	4 6 8 5 7 2 7	39	7
Goat	Goat (preceded by these crossed out words: 'I'm a goat sheep with' and followed by the crossed out word: 'tendencies')	4 6 2 10 2 5 8 7 7	51	9

TABLE 3¹

SERIAL TEST OF TARGETS IN THE SEMESTER BEGINNING
FEBRUARY 1951

The serial test examines the targets to find how often each symbol (or colour) is followed by every other symbol (or colour). Obtained frequencies are evaluated against theoretical frequencies in a 5 × 5 table.

Session	Targets Composed of Symbols			Targets Composed of Colours		
	Number of decks	Chi square (04 d. f.)	P	Number of decks	Chi square (24 d. f.)	P
1	4	30.75	.162	4	19.50	.725
2	4	8.50	.998	4	15.00	.921
3	4	22.00	.579	4	32.00	.127
Total	12	22.333	.559	12	18.333	.786

¹ Betty and Fraser Nicol planned and performed the test reported in this table; and I wish to express my gratitude to them for doing so.

TABLE 4¹

GAP TEST OF TARGETS IN THE SEMESTER BEGINNING FEBRUARY 1951

The gap test examines the targets to find if the number of steps between successive appearances of a given symbol (or colour) conforms to theoretical expectation. Because the expectations for many gap levels are small, entries were pooled for Gaps 3 and 4, for Gaps 5, 6 and 7, and for Gaps 9 and above. Thus each chi square has six degrees of freedom. (For the three individually considered sessions, expectations are too small to warrant separate treatment.)

Targets Composed of Symbols ($N=12$)			Targets Composed of Colours ($N=12$)		
Symbol	Chi Square (6 d. f.) ²	P	Colour	Chi Square (6 d. f.)	P
Circle	7.744	.26	Blue	5.3849	.496
Cross	8.926	.18	Brown	3.0876	.798
Square	10.965	.092	Green	2.3066	.89
Star	2.208	.90	Red	6.7389	.35
Wave	4.014	.68	Yellow	2.6983	.84

¹ Betty and Fraser Nicol planned and performed the test reported in this table; and I wish to express my gratitude to them for doing so.

² The Nicols add: 'The chi squares here require some correction because of the fact that four runs consisted of closed packs, while the rest were open. But we doubt that the correction would seriously alter the results.'

TABLE 5

FREQUENCY OF TARGET ITEMS USED IN GROUP EXPERIMENTS

Targets Composed of Symbols					Targets Composed of Colours			
Symbol	Closed Decks $N=149$	Probably Incorrectly Transcribed Closed Decks $N=16$	Open Decks $N=222$	Total Decks $N=387$	Colour	Closed Decks $N=8$	Open Decks $N=24$	Total Decks $N=32$
Circle	745	83	1105	1933	Blue	40	124	164
Cross	745	72	1095	1912	Brown	40	133	173
Square	745	84	1106	1935	Green	40	107	147
Star	745	82	1109	1936	Red	40	122	162
Wave	745	79	1135	1959	Yellow	40	114	154
Chi square (4 d. f.)		1.175	.804				3.285	
P		.90-.80	.95-.90				.70-.50	

TABLE 6

SEQUENCES OF IDENTICAL ITEMS (PATTERNS OF AA, AAA, ETC.) IN
TARGETS FOR GROUP EXPERIMENTS¹

A. Targets composed of Symbols

Symbol	Closed Decks N=149				Probably Incorrectly Transcribed Closed Decks ² N=16			Open Decks N=222						Total Decks N=387					
	Sequence				Sequence			Sequences						Sequences					
	2	3	4	5	2	3	4	2	3	4	5	6		2	3	4	5	6	
Circle	122	13	1	0	14	4	0	213	40	6	3	1		349	57	7	3	1	
Cross	119	23	0	0	7	2	1	216	57	14	3	0		342	82	15	3	0	
Square	127	14	2	1	14	3	0	231	51	8	2	0		372	68	10	3	0	
Star	129	15	2	0	8	0	0	223	36	4	1	0		360	51	6	1	0	
Wave	103	9	0	0	11	2	0	235	44	13	4	2		349	55	13	4	2	
Expecta- tion	119.2	14.9	1.3	.06	15.4	2.9	.6	213.1	40.8	7.8	1.5	.3		347.7	58.7	9.7	1.7	.3	

B. Targets Composed of Colours

Colour	Closed Decks N=8			Open Decks N=24				Total Decks N=32			
	Sequence			Sequence				Sequence			
	2	3	4	2	3	4	5	2	3	4	5
Blue	5	0	0	20	4	1	0	25	4	1	0
Brown	9	3	0	28	4	0	0	37	7	0	0
Green	9	1	0	17	2	0	0	26	3	0	0
Red	10	3	0	23	5	2	1	33	8	2	1
Yellow	5	1	0	28	7	1	0	33	8	1	0
Expectation	6.4	.8	.1	23.0	4.4	.8	.2	29.4	5.2	.9	.2

C. Summary

Closed Decks	Sequences				
	2	3	4	5	6
Obtained	638	82	5	1	
Expected	628	78.5	6.8	.3	
All other Decks					
Obtained	1288	261	50	14	3
Expected	1257.6	241.0	46.1	8.8	1.7
All Decks					
Obtained	1926	343	55	15	3
Expected	1885.6	319.5	52.9	9.1	1.7

¹ These sequences are cumulative when the table is read from right to left. Thus AAAAAA is given two entries in the 4 column and also three entries in the 3 column and four entries in the 2 column.

² Expectations were calculated as if these were open decks, since technically they were not closed. If they are treated as closed decks, the expectations are 12.8, 1.6 and .1, and expectations for Totals are correspondingly altered.

REVIEWS

EXTRASENSORY PERCEPTION AND PERSONALITY PATTERNS. By G. R. Schmeidler and R. A. McConnell. London, Oxford University Press, 1958. 136 pp. 25s.

Among books dealing with particular series of experiments in parapsychology this could make a strong claim to be the best yet published.

The backbone of the book is an account of Dr Schmeidler's investigation of the relation between *belief* in ESP and *success* in ESP. Between 1943 and 1951 Dr Schmeidler collected, from subjects working individually or in groups, nearly 300,000 guesses at ESP cards. Each subject rated himself as either a sheep or a goat, sheep being people who thought the given ESP task possible or who were uncertain, and goats those who thought it impossible. The task was usually 'clairvoyance' and the subjects were university students. (The ratio of sheep to goats varied widely from one term to the next: it would be interesting to know why.) Analysis showed that sheep scored very slightly better than goats, to the extent of 1 extra hit per 68 guesses in the individual tests, and 1 per 147 guesses in the group tests. The total number of guesses was so large that this small difference is significant at the level of one chance in ten million, so the results cannot reasonably be attributed to chance.

After two opening chapters introducing the subject of ESP research, Chapters 3 and 4 describe the individual tests and Chapter 5 the group tests.

Chapter 6 attempts to explore more deeply the nature of the effect. If sheep score high and goats low, there should be some point in between where the score switches from positive to negative. Belief and disbelief in ESP do not form a sharply defined pair of alternatives; many intermediate attitudes are possible, such as agnosticism, belief in only certain kinds of ESP, belief that others can do it but not oneself. How are these intermediate cases to be classified? Fortunately records exist of experiments by many different investigators, each of whom seems to have chosen (apparently somewhat arbitrarily) a different way of classifying belief in ESP. Chapter 6 analyses and compares these findings. The result is disappointing. Apart from a general tendency for sheep to score better than goats there is no consistency between different investigators in the results for the intermediate categories of belief. The authors conclude that there are other relevant variables at work, among which they suggest as particularly important the personality of the experimenter. This hypothesis is

reasonable in itself, but the authors might perhaps have acknowledged that, pushed only a little further, it could undermine the main sheep-goat finding itself. If the results for intermediate beliefs can be explained away as due to the personality of the experimenter, then so can the results for sheep and goat beliefs. In particular it might be that sheep score higher than goats simply because, ever since Dr Schmeidler's first few exploratory runs, all the experimenters have always expected them to.

The authors feel that the ideal sheep-goat classification would be based on the attitude to the actual task in question. They are conscious of the difficulty of measuring this attitude reliably, both because of varying interpretations by different people of the words used and because of varying moods in the same person. One might have thought that these difficulties could be largely met by renewing the attitude assessment at each run and basing it on the question: 'How many successes do you expect to get?' This question appears to have been asked in some exploratory tests (p. 97), but the theoretical arguments in its favour do not seem to have been appreciated. Was it found unsuccessful in practice? The matter would have been worth discussion in Chapter 6.

We may mention here the fact that the correlations with sheep-goat attitude do not necessarily imply that this attitude has anything more than an incidental relation to ESP scoring. The true determinants might be something of a totally different kind, though slightly related to the sheep-goat attitude. Considering the very low level of the sheep-goat/ESP correlations and our almost complete ignorance about ESP, this warning is important and might well have been given greater stress in the book. For all the evidence presented, the true determinant of ESP score might be such a humdrum variable as age, sex or race.

Chapter 7 briefly reviews some similar work in which subjects were also given the Rorschach inkblot test. Unfortunately the main work is, apparently, subject to criticism on technical grounds (exactly what grounds is not specified) and the full results are therefore to be held back for later publication. The main provisional conclusions, stated with some reserve, are that the sheep-goat difference is stronger for well-adjusted subjects, and that, over and above any other effects, the positive ESP scorers are characterized by a more open-minded readiness to accept what is put before them.

Chapter 8 describes a series of ESP tests carried out by Dr Schmeidler on patients suffering from cerebral concussion. It is a pity that the experimental controls here were weaker than they need have been (the same experimenter both prepared the targets

and recorded the guesses), for otherwise these are some of the most interesting results in the book. The concussion patients scored significantly high, and significantly better than a control group of patients without concussion. The tentative conclusion is again drawn that an attitude of passive receptivity favours positive scoring.

Chapter 9 reports work with another personality test measuring response to frustration. Here the sheep-goat effect is being studied again in a more subtle form, and again it is found that subjects who react aggressively against the experiment tend to score negatively. The level of statistical significance is, however, little more than marginal.

Chapters 10 and 11 summarize the findings and discuss their implications. Three appendices add further details about the experiments and examine some of the statistical problems. One commendable innovation in the statistical treatment of the sheep-goat results should be particularly mentioned. In the past such data have often been analysed by testing the significance of the difference in the mean scores of sheep and goats, using the standard error obtained from the theoretical binomial distribution. This has been rightly criticized on the grounds that it allows for sampling error only in the ESP test scores and not in the sheep-goat classification, and it has been argued that the correct procedure is to test, by chi-squared, the association between the two dichotomous classifications high/low score and sheep/goat attitude. But this is seriously insensitive. Fortunately the authors have been astute enough to realize that the difficulty can be overcome, with no loss of sensitivity, by using the analysis of variance with the between-subjects variance estimate in the denominator of the F-ratio.

The book is clearly written and methodical. The authors are exceptionally conscientious and the level of scholarship is markedly higher than usual in a book about ESP.

Four criticisms may be mentioned. The first and second of these can be answered readily, and are only mentioned here because they are often raised by critics. In the opinion of this reviewer it is a pity that they were not answered in the book. The third and fourth raise serious doubts on the validity of the main results. Whether they can be answered only the authors can say.¹

1. *Low validity of personality tests*

Personality tests, and particularly the Rorschach, have a poor reputation for validity among many psychologists, and one some-

¹ See comments by Dr Schmeidler and Dr McConnell on pp. 63, 79 of this number.

times hears it argued, 'How can we be expected to believe that the Rorschach is predictive of ESP scoring when it has not been shown to be predictive of anything else of any importance in the human personality?' This argument is unjust, for it overlooks the very small level of the ESP personality correlations reported. Thus a typical difference between mean ESP scores of those with favourable and unfavourable personalities would be less than one-tenth of a standard deviation. This corresponds approximately to a biserial correlation coefficient of .05. Even the strongest critics of personality tests would hardly wish to insist that their validity cannot reach to this level.

2. Effect on the overall distribution

If sheep are scoring above chance and goats below, the score of the two groups pooled may be on expectation. It has been argued that this explains the frequent failure to observe any ESP effect in a mixed bunch of percipients. Critics, however, rightly point out that, with samples of subjects drawn in the very irregular manner which is customary, it would be very odd if the sheep and goat effects always just cancelled out. Indeed, one has heard it argued that they so often do that this is not merely odd but suspicious. The argument is theoretically correct but again overlooks the quantitative magnitudes. The sheep and goat effects are both very slight. When sheep and goats are pooled the balance is necessarily slighter, and only a very long series would reveal its deviation from expectation. In fact if we take all the results of all the sheep-goat experiments reported in the book (Chapters 4-6) we do find a significant positive deviation ($C.R. = 2.7$) after pooling sheep and goats, so that at least for these data the criticism is answered.

A similar point may be made about the variance. If sheep score high and goats low the variance of the total group should be above its chance expectation. If it were found not to be it would be almost impossible to accept the genuineness of the ESP effect. In the individual tests the variance was in fact found not to differ significantly from expectation. However the critic can be answered by showing that the anticipated effect on the variance would be so small that it would not be significant for a series of the length used.

3. Retrospective rejection or reclassification of subjects

In the personality-ESP enquiries it appears to have been usual for the personality test to be administered after at least some of his ESP scores had been made known to the subject. The standard of experimental rigour exhibited in most of the work described in the book is generally so high that one hesitates to suppose that such an

obvious error really occurred. But it seems quite clear that something similar happened at least sometimes in the sheep-goat experiments, for we read on page 113 that subjects who changed their minds about their sheep-goat classification during the session were rejected from the analysis, and we know that subjects were in many cases told their results during the session, and hence possibly before changing their minds about E.S.P. It seems obvious that sheep who find they are scoring low and goats who find they are scoring high will be the most likely to change their minds, and if some of these are then deliberately excluded from the analysis the remaining sheep will naturally show a trend towards high scores and the remaining goats to low. This error could therefore entirely invalidate the findings of the book.

Similar trouble might arise through ambiguous classifications, and the subjective judgments involved in deciding whether they are ambiguous (or even whether to look for an ambiguity). Indeed, if we refer to one of the original reports (*Journal A.S.P.R.*, 37, 1943, p. 217) we find that four subjects have been reclassified and then omitted from the analysis. If they are put back in, according to their original classification, the whole of the required negative deviation for goats in the three series concerned disappears.

Even if the ultimate classification of all ambiguous cases was made without knowledge of the E.S.P. scores there is still room for error. Thus it is not too fanciful to imagine that the experimenter may have made a special examination of cases which failed to support her hypothesis (i.e. of low-scoring sheep and high-scoring goats). In the course of this examination she may have come across some previously unnoticed ambiguous classifications. If these were presented (as was the rule) to a judge who had no knowledge of the E.S.P. scores he will almost certainly have changed some of their classifications. Unless equally careful examination was made of cases which supported the hypothesis, this procedure will bias the results towards the hypothesis.

The authors describe a number of precautions designed to guarantee that the sheep-goat classification was not influenced by the E.S.P. score, but they do not meet the above objections.

4. Target randomization

The targets in the main series were prepared by a paid assistant from published random number tables, by a simple coding procedure in which each E.S.P. symbol was represented by either of two different digits. Nothing is said about any checks on this procedure nor about any tests for randomness on the target

sequences as used. If the assistant's work was not checked there are certain to have been clerical errors and it is very unlikely that these would not effect the randomness of the target sequences.

Now unless the guess sequences show almost fanatical bias, large departures from randomness in the target sequence are needed in order to produce a modest effect on the scores. However, in the present work the ESP effects reported are so slight that a fairly small departure from true randomness might explain them away. Suppose, for example, that the assistant sometimes wrongly coded the digit 0 into the symbol 'circle' instead of the correct 'star'. Her task was repetitive and tedious, it demanded considerable concentration, and she was not being checked. Under such circumstances some people perform well, some surprisingly badly, and it does not seem to strain credulity too far to suggest that she may have made this mistake on 2 percent of all occasions, or 20 percent of the zeros. Further, it is known that different people have different guessing preferences, and it is quite reasonable to expect that these preferences might reflect, in some measure, their personality. Suppose the open-minded, co-operative sheep have a preference for the open-minded friendly symbol of the circle, to the extent of guessing circle thrice, instead of twice, in every 10 guesses, and a counterbalancing bias against the jagged, aggressive star. Under these circumstances the expected sheep score, on a chance basis, works out at 5.1 — just the figure observed in Dr Schneider's group experiments. For the goats we should have to assume a similar bias in the opposite direction. These assumptions are far-fetched, certainly, but not to the point of absurdity. A simpler theory (which would also be simpler to refute by tests for randomness) is that the assistant, like an assistant of Dr Soal's entrusted with a similar task, deviated in some radical manner from her instructions, to the extent, for example, of making up the target sequence out of her own head. Of course these particular suggestions should not be taken very seriously (there is an infinity of others that could be devised) but they serve to show how some phenomena whose existence is already plausible and whose magnitude need not be impossibly large would suffice to explain away entirely the experimental results reported in this book.

At this stage the best that could be hoped for in answer to this criticism would seem to be a thorough series of tests for randomness, both for single symbols and for certain other psychologically likely patterns unsymmetrical with respect to the symbols.¹ Have any such tests been performed?

¹ Biases affecting all symbols symmetrically would affect only the variance, not the expectation, and so would not require investigation.

To sum up, this is an admirable book in nearly every respect. But the reader should bear in mind that the results described could be fairly readily explained without recourse to the hypothesis of extrasensory perception.

CHRISTOPHER SCOTT

COMMENTS BY DR R. A. MCCONNELL

PERHAPS I can add some useful information relating to Mr Scott's criticisms appearing in the section, 'Retrospective rejection and reclassification of subjects.'

The kind of sampling check which I reported in Appendix B of our book would have obviously failed in its stated objective if I had not considered the question of retrospective rejection of subjects. At the time of my visit to the City College of New York described in the book on p. 125, Dr Schneider explicitly assured me that I had found in her files and held in my hands *all* of the ESP group-data that had been gathered in the two semesters beginning September 1949 and February 1951 and that none had been rejected for any reason.

The question of the classification of ambiguous subjects lay outside the responsibility I had assumed, as is stated on p. 124 of our book. Nevertheless, for my own satisfaction I did go into the possibility of bias through misclassification of such cases, and this would include Mr Scott's hypothesis of selective reclassification. My records show that for the semester beginning September 1949 the nature of the mimeographed sheep-goat questionnaire used was such that by my actual inspection there were no cases where an ambiguity of classification needed to be resolved. Hence, for this semester I cannot entertain Mr Scott's speculation regarding bias by independent reclassification of only high scoring goats and low scoring sheep. In the semester of February 1951 the classification form was not used and no simple conclusion could be drawn from the objective evidence that I inspected.

I presume that with regard to these and other questions raised by Mr Scott, my co-author is providing detailed and more embracing information.

THE EASTER ENIGMA. By Michael C. Perry. London, Faber & Faber, 1959. 243 pp. 21s.

Mr Perry, lately Senior Scholar of Trinity College, Cambridge, has given us a book which is well worthy of study. He writes from the Christian standpoint and addresses his book mainly to those

logical students and students of psychical research. Coming from the field of theology it is refreshing to find one so well informed on the subject of psychical phenomena.

An introduction has been written by Austin Farrer, Fellow of Trinity College, Cambridge, with the timely reminder that it is inexcusable for the Christian enquirer of today to leave psychical research out of his reckoning. Mr Perry is well aware that this is particularly true in the case of the Resurrection.

The book is divided into two parts. The first is largely an examination of the Biblical narratives. He believes that all 'explanation' of the Resurrection is part of God's working *within* the laws of nature. Regarded as an isolated fact it will be suspect, but it increases in value if similar phenomena are known to science.

The first question considered is Did It Happen? There follows a careful examination of the evidence with chapters on the Death of Christ and the Empty Tomb. 'It was not the empty tomb on which the proof of the Resurrection rested, though the empty tomb would be a useful corroboration.' When considering Christ's Resurrection and Ours he draws the difference between the Greek and Hebrew ideas and rightly dismisses as untenable the resurrection of the flesh, which is not taught in the New Testament. There is a resurrection of the body but Paul distinguished between 'body' and 'flesh'. Because this distinction was not made his doctrine was distorted by the early church. After comparing recorded cases of hallucinations with the Resurrection he concludes that the disciples were not deluded. 'We are left with the theory that the Resurrection was a message from a source external to the apostles.'

The second part of the book discusses cases of veridical apparitions and the telepathic correlation between agent and percipient. There is a difference between experimental and spontaneous cases. In the former 'the agent *must* initiate the hallucination by means of an act of will directed to the percipient'. In the latter it comes automatically and astonishing to both. In both cases, he holds, there is an agent even though the agent is no longer alive. The survival hypothesis is reasonable and, 'granted survival, it *does* then become possible to say that the term "veridical" as applied to apparitions of the dead means that the ostensible communicator was the actual communicator.' Recorded cases of materialization are unconvincing to the author. He believes the Resurrection to be more in keeping with spontaneous apparitions of the dead. The telepathic theory offers the most promising approach towards the understanding of Christ's appearances. This, he thinks, cannot be readily dismissed even though it does not cover the whole

truth of the event. Scriptural references which suggest physical contact with Christ's risen body are examined in detail. The manner of the disappearance of Christ's physical body suggests dematerialization.

Throughout the book the author keeps in mind the deeper issues of the Christian Faith. The Christian hope is not mere survival but immortality and eternal life. Christ conquered death, and faith is trust in a living person.

The book contains a useful bibliography and index for the student. It is sure to provoke controversy but whatever one's opinions the author has produced a stimulating book worthy of study, and has made a valuable contribution to a difficult subject.

W. H. STEVENS

PSYCHICAL PHENOMENA. By Fr Reginald-Omez, O.P. London, Burns & Oates, 1959. 7s. 6d.

This is a translation by Renée Hayes of *Supranormal ou surnaturel* by a French Father of the Order of St Dominic, the author of numerous articles and books on psychology, parapsychology and related subjects. It is No. 36 of a series bearing the antithetical general title of 'Faith and Fact Books'. The translator quotes with approval the Italian proverb 'Translators are traitors'—either to the author or the reader in whose language they write. One would judge, however, in this effort she has avoided successfully the double treachery of misrepresenting the one and misleading the other. She has added to the text numerous valuable explanatory footnotes throughout the book sometimes modifying, or even disagreeing with, the conclusions of the author.

The first section is an historical sketch of Psychological Research written largely from the French background and built mainly on the work of Charles Richet, Geley, Osty, Warcollier and Robert Amadou. Next follows a section dealing with the subject matter and methods of parapsychology with pages dealing with illusory hallucinations, the morals of hysteria and methods of fraud and cheating. One is impressed by the account of the careful honest investigation of the Church authorities in such cases of paranormal phenomena as occur, for example, in the 'miracles' of the Lourdes cures. But to many the most interesting section will be Chapter III which discusses the relation between Psychological Research and the Catholic Faith.

Facing the table of contents on p. 4 appear the words 'Nihil obstat : censor deputatus'. That would seem to indicate a change in the Catholic outlook since the days of Copernicus. On p. 96

the author states: 'The Church has no cause to intervene in the purely scientific domain of the studies in the paranormal *so long as those engaged in them remain on their own ground*' (my italics). Nihil obstat—so long as any conclusion reached falls within the framework of the obligatory teaching of the Church. Thus the author states (p. 98) 'parapsychologists must not deny the freedom of the will'. They must not 'set forth doctrines concerning the fate of the dead' or maintain that the dead 'really intervene as a result of evocations other than prayer'. But within the proper sphere of investigation—nihil obstat.

This one feels is a wrong approach. The true investigator should, as our Society's founders insisted, examine 'without prejudice or prepossession those faculties of man, real or supposed, which appear to be inexplicable on any generally recognized hypothesis'. But that is the ideal attitude difficult to achieve. Are there not shackles that bind us, often unsuspected, other than those of the dogmas of any Church? It is difficult for anyone to free himself completely from the effects of the climate of materialistic philosophy in which one has been bred.

The author accuses J. B. Rhine of 'trespassing into the spiritual sphere as a destroyer of all the most essential values of religion' but the translator, in a footnote (p. 78) points out that the French translation of Dr Rhine's *The New World of the Mind* as quoted by Fr Reginald-Omez is a good deal more anti-religious than the original American text and, moreover, the bitter Communist attack on Rhine and his conclusions hardly support the view that they are essentially anti-religious.

Ch. IV summarizes the conclusions of contemporary scientific opinion on both the alleged physical and psychological phenomena of psychical research and a final chapter discusses its position as a science of the future. Investigators 'can help in the struggle against superstition and occultism, against the preoccupation with the marvellous that can crowd out true religious feeling'.

There is a select Bibliography of about twenty odd names—among them apparently only two of the Catholic Faith.

A sincere book, a sincere author and a sincere translator.

G. W. F.

THE JOURNAL OF PARAPSYCHOLOGY, XXII, No. 3, September 1958,
Durham N.C.

Margaret Anderson and Rhea White report further experiments on the relation between teacher-pupil attitudes and the level of ESP scoring when the teacher acts as agent. The results suggest

that the scoring level is not affected by the teacher's attitude to the pupils but they indicate at no very high level of significance that it is affected favourably by a positive attitude of the pupil to the teacher.

There is also a report in the same field of study by Christiane and Paul Vasse. Again the teacher acted as agent; in this case the teacher was C. Vasse herself. The results were positive and highly significant but although they contribute to the evidence for ESP in this relationship, these experiments do not seem to have been designed to add anything new to our knowledge of ESP. They do however confirm an observation, also reported by the authors of the earlier article, that the children score better at the end of a term with the teacher than they do at the beginning.

Professor Rhine discusses the implications of the Unconsciousness of psi. He finds an essential difference between ESP and sensory perception in the fact that the former has no distinctive modality and must therefore be conveyed to consciousness by one of the various modalities of sense experience. It is true that the experient may have a justified conviction of the genuineness of a psi experience, but this conviction is not awareness of ESP as such. Rhine suggests that these considerations have bearings on the various attempts which are now being made to achieve control over psi abilities.

C. B. Nash's article on 'Correlation between ESP and religious value' reports two findings which the author rightly regards as merely suggestive. The first is that his subjects seemed to show negative correlation between the scores they predicted for themselves in ESP tests and the scores they actually obtained. The second is that there seemed to be a positive correlation between ESP scores and the results of a test of religious values. Neither finding can be regarded as established without further confirmation.

There is a discussion by R. A. McConnell of criticisms made by Dr L. G. Humphrey of McConnell's PK experiments. He does not accept Dr Humphrey's view that the data may be of altogether chance origin, or that the apparent departures from chance expectation are non-psychological in their characteristics.

There is a sympathetic review of Schmeidler and McConnell's important book on *ESP and Personality Patterns*. It is unfortunate, however, that the reviewer does not seem to have read the book with sufficient care to realize that the estimates of significance reported on p. 46 were calculated by the analysis of variance and are therefore not open to the criticism he makes of them.

R. H. THOULESS

THE MIND READERS. By S. G. Soal and H. T. Bowden. Faber and Faber, 1959. 292 pp. 30s.

Has Soal done it again? First Shackleton, then Mrs Stewart and now, to complete the hat-trick, two adolescent boys producing scores of a level unprecedented in any well-authenticated experiments. This book unfolds the dramatic story of an investigation which lasted from August 1955 till April 1957. It is written by Dr Soal. Mr Bowden's name appears on the title page because he contributed nearly as much as Soal to this investigation. The percipient, Glyn Jones (born in April 1942), and the agent, Ieuan Jones (born in August 1942) are cousins, though not, we are told, particularly close friends. Their families live in Snowdonia, in cottages two or three hundred yards apart. They are typical Welsh-speaking mountain people. Ieuan's father, Will, is a labourer for the Forestry Commission; Glyn's father, Richard, is 'shift-boss' in a lead-mine, and his wife runs a café in the front room of their cottage. Soal has known both families for thirty-six years: since 1923 he has been spending climbing holidays at Will's cottage. In August 1955, he tried some informal card-guessing experiments with the children of the two families, but the first few gave only chance scores. His interest was aroused when Glyn got marginally significant scores, first with Rowenna, his cousin, and then with Ieuan as agent. To encourage them he offered rewards: each boy would get 1s. for 9 hits in a run, 2s. for 10, 4s. for 11, and so on in geometrical progression. He was caught out, for at the end of this holiday he owed them over £15! He then revised his scale as follows: 6d. for $9 \times 6d.$ to 4s. 6d. for 17, then £1 for $18 \times 10s.$ to £4 10s. for 25. He could scarcely have foreseen that even on the new scale, the boys would earn large sums. This policy of payment by results may be open to criticism, but Soal does I think meet it (pp. 11-13 and 21). Both experimenters are convinced that without this incentive the boys, who had no interest in telepathy as such, would not have sacrificed so many weekends and holidays to these monotonous experiments. The most notable feature of this preliminary work is that Glyn twice got significant scores under clairvoyance conditions (77/200 and 52/125). Soal expresses bitter regret that he left Wales without doing more clairvoyance tests with witnesses, for subsequently all clairvoyance tests yielded chance results. On his return to London, Soal asked Bowden to visit them alone to try to confirm his findings. Bowden did so. The score he obtained, ignoring runs when a member of the Jones family acted as signaller, was 120/350 (average 8.57 per run).

It may be helpful if I now describe in general terms the method

which was followed in the subsequent experiments. Closed packs (i.e. 5 cards of each of the 5 symbols) were used throughout. One experimenter (EP) sat beside Glyn (P). Another experimenter (EA) sat with Ieuan (A) and showed him the cards. A screen blocked vision between P and EP on one side and A and EA on the other side. To synchronize guesses, there was a signaller (S), normally a different person from EA. S stood (sat) where he saw EA showing a card to P, but could not see the face of this card, and his job was to call 'next (guess)' as a signal for P to make his guess. (In some indoor experiments, which were equally successful, the signaller used an electric bell instead of calling.) I will not elaborate here on the precautions which Soal took to eliminate the possibility of sensory cues or fraud. Suffice it to say here that he acknowledged a duty to meet Dr George Price's challenge by devising conditions which made it impossible for the regular experimenters as well as the subjects to cook the results. This was done, in many of the open-air experiments, by getting visitors to perform the roles of EA and S. At one such experiment, I acted as EA and my University colleague Dr Whitehead acted as S (pp. 164-170, and 278-9). I selected a pack of cards from a bag of about 20, shuffled and cut it, showed the cards to A, and retained the pack until the target-sequence had been copied on to the score sheet, while I checked each entry. I was completely satisfied that A gave no visible or audible signals, and I put this in writing at the time. The score was 85/150. Many other people were given similar opportunities to supervise experiments, and have recorded similar verdicts. Dr Rhine, in his salty contribution to Appendix O, asks 'who else . . . has ever rounded up so many excess witnesses, each labelled conspicuously with all his university degrees and positions of importance, and each leaving his testimonial as he departs?' (p. 286-7).

I return now to the history of the case. In September 1955, Mrs K. M. Goldney joined Soal and Bowden in a week-end visit to Wales and conducted a series of tests under stricter conditions than had previously prevailed. For this visit the total telepathy score was 575/1625 (average 8.84 per run). The next step was a visit to London, in October, when a series of experiments were carried out in the Library of the (former) S.P.R. rooms. High scoring was witnessed by Mr Fisk, Professor Mace and Dr Thouless. The visit yielded an average score of 9.28. One noteworthy feature (it recurred later) was that when A and P were in adjacent rooms but in direct line with an open door they scored high, but when one of them was moved a few feet to the side the scoring dropped to the chance-level (p. 61). 'An Unfortunate

Episode' is the title of Chapter VI. Soal, Bowden and Goldney visited Wales in November, 1955. At the first session (a.m. 5 November) the boys employed a system of auditory signalling. It was noticed that: 'From the end of the very first run Ieuan started to cough at fairly regular intervals, though he showed no signs of a cold.' K.M.G. (EA) who consequently 'stood where she could purposely see the faces of the cards, noticed that on three occasions when LION was shown to Ieuan he creaked his chair and that on all three occasions Glyn's guess was right. Then S.G.S. noticed two sniffs on occasions when PENGUIN was shown and these also turned out to be correct' (p. 69). Yet the scores for this sitting were only 3, 5, 3, 9, 9. After this session, Soal complained to the parents about Ieuan's noisy behaviour, but did not use the word 'fraud', and 'both boys were given a good dressing down by their fathers'. Later that day some very high scoring was achieved—the best scoring yet achieved (164/300) and most of the runs were made with the boys in adjacent rooms *with the door shut*. The experimenters were satisfied that no code was then in operation: 'Ieuan now sat perfectly still; there was not a single cough, sniff or chair-creak.' Another successful sitting was held that evening, in my own presence, again with the door shut, and I am certain that no code of the kind described above was in use. But this was not the end of the cheating. On their next visit, in December, Soal and Bowden were accompanied by Mr F. Bateman. After two sittings when only chance scores were obtained, Ieuan gave 'a performance as crude as it was farcical', employing a code consisting of coughs, stamping, and chair-creaking. Bowden (EP) in the next room asked what all the noise was about. The score, however, was only 41/125. Soal said nothing to the boys but explained later to Bowden and Bateman what was going on. To let them see for themselves, the boys were allowed to repeat the performance. Bateman, who had been absent at the first performance, identified the code during the first run, and, using it, scored 20 for the second run (Glyn's score was 21). That night Soal asked Glyn when they started 'this silly cheating', and Glyn confessed at once. He denied however that they had ever used a code except that day and on the morning of 5 November. This episode nearly led Soal to abandon the experiments, but in fact I think it adds to the authenticity of the report. The crudity of the boys' attempted deception provides a very strong argument against their having used, or been capable of devising, a system which escaped detection by the host of experienced investigators who witnessed their performance. (More on this later.)

In April 1956, after a four-month lull, Soal revisited Wales with Bateman. On the first day, two normal sessions gave high scoring (average 8.5), and there were no signs of cheating. Next day a surprise was sprung on the boys, an experiment with fresh cards, using symbols they had not seen before. The cards were not shown to the boys till the beginning of the experiment, throughout which the boys were not allowed to communicate with each other. This sitting gave a high score from the first run—10, 8, 14, 15, 14, 12, 10, 7=90/200. Soal comments: 'The immediate success of this experiment makes it difficult to suppose that a code could have been employed', particularly since analysis showed no significant tendency for Glyn to score on one symbol more than on another. The subsequent work in this visit was devoted chiefly to studying the effect of opening and shutting a door between A and P. Soal found (pp. 95-100) that when the boys were scoring high with the door open, the score dropped to chance when the door was shut and also when it was only 2 inches ajar or quarter-open. Soal attributes this to a psychological inhibition, and comments that when the door was quarter-open, 'acoustically, the situation is scarcely different from that in which it is wide open' (p. 99). Similar results were obtained in another experiment next day, but an incidental feature of this experiment must be mentioned. The door had just been re-opened and the score was expected, as in previous cases, to shoot up, but Soal made Ieuan keep his hands over his mouth while he looked at the cards: and a chance score was obtained. (Later on I discuss the possible significance of this.)

The next development was the introduction of out-door tests. The experimenters were anxious to accustom the boys to work at increasing distances, but felt that this must be done gradually. The first open-air tests were in Wales (April 1956), and gave good results at 52 and 69 feet (average score 10.75). Open-air tests were also done that May in London, and the second of two sessions produced high scores at 69 and 99 feet. The climax came during the visit by Bowden, Soal and his brother C. W. Soal from 27 July to 10 August. The scoring level now took an upward leap. Up to now, 19 was the highest score achieved (thrice), but during this visit scores in the 20's became quite frequent (Glyn twice got 25, 4 times 24, and 26 times got 20 or more). The average score, for the 77 telepathy runs done during this period, was over 13 per run. There were several short sessions of 4 or 6 runs, supervised by outside witnesses, each of which produced a result whose statistical significance is equivalent to that achieved by Basil Shackleton in two years' work (e.g. 82/100 in the presence of Mr O. W. Owen, a lecturer at a Technical Institute). It is not

surprising that, confronted with this unprecedented scoring-level, Soal's main concern was to eliminate the sort of criticisms Dr. Price has made, rather than to try to learn about the relevant cause-factors by varying the conditions. He did however accustom the boys to work at greater distances up to 166 feet, and this involved weaning them away from the method, which *they* felt was necessary, by which P called his guess aloud and A thus learnt the result of each trial. The most notable of the tests in this period was the bathing costume experiment on 10 August. Suggestions had been made that the boys might be communicating by means of miniature wireless sets. The experts consulted did not think this practicable, but Soal set out to eliminate it by getting the boys to perform in close-fitting bathing costumes, and without shoes. The first two runs gave chance scores, and Soal, getting anxious, begged the boys to do their best, and there followed 20, 20, 23 and 20. Soal reports that immediately after the last run he 'felt all over Glyn's bathing costume and his socks but there was nothing concealed. S.G.S. felt the insides of the boy's arms and thighs and looked in his ears. He then ran to Ieuan (who had been under the close observation of C.W.S. since the end of the final run) and examined his costume and socks but again drew a blank' (p. 158).

The next landmark was the third visit to London at the end of August 1956. Soal reports a comment by Professor Stratton that the opinion of Mr Jack Salvin (Chairman of the Occult Committee of the Magic Circle) would be worth more than the testimony of half a dozen Fellows of the Royal Society. Salvin was approached and he agreed to spend a few days in London investigating the boys, at a time, however, when Soal was not free to participate. Although at first reluctant, Soal agreed that Bowden and Mrs Goldney should accept Salvin's offer. Seven sessions were held in the S.P.R. Library. 'Before starting, K.M.G. impressed on Salvin that the conditions were to be entirely in his own hands' (p. 176). The first three sessions were strikingly successful, the boys maintaining the level of scoring of the recent open-air tests (averaging 13.3 per run). The last four sessions were complete failures so far as the scoring was concerned, but they, too, contributed to the purpose of the series, for Salvin deliberately left loopholes in the conditions, which the boys made no attempt to exploit. The boys ascribed the breakdown after the third session to their disappointment because a proposal that they should give a demonstration at a meeting of the British Association proved impracticable. Salvin's verdict was emphatic: 'I am completely satisfied, after making all the observations I desired and having permission to do what I wished, that no code or trickery took place... and, in fact, that code or

trickery in the experimental conditions I witnessed was impossible' (p. 178). Also present at the first two sessions, and acting as EA in the second, was Mr A. Reeves, Director of the auditory research laboratories of Standard Telecommunications Ltd. Reeve's testimony is printed, along with a letter in which he describes the theoretical considerations which led him to his conclusion that no auditory signalling took place. He says that these considerations would no longer apply if the critics assumed that the boys were 'freaks of nature talking to each other at above 20 k.c. or more'; but, he concludes: 'perhaps the best counter to this "super-sonic" assumption is the fact that no tendency has been observed for the inverse-square law to be fulfilled' (p. 188).

We must now take note of a storm which had been brewing. Throughout these experiments much of the experimenters' energy had been absorbed in keeping the peace among the Jones families, smoothing out tiffs and quarrels. The Jones' were emotional and quick to take offence, Glyn being particularly temperamental. (Some friction was inevitable. It cannot have been easy for the Jones' to have their very limited living-space monopolized so often by the experimenters; and the latter drove their subjects pretty hard; e.g., on one week-end visit, Ieuan was roused from bed for a session after 10 p.m., and next day 41 runs were done!). But the rows were short and quickly forgotten—until a reporter from a less scrupulous Daily got busy (pp. 171–2). In the attempt to wheedle a story out of them, the parents were led to believe that it, and the boys' powers, could earn very large sums; and the suspicion was fostered that Soal was exploiting them and was going to make a packet out of them. It was in this frame of mind that the Jones' cancelled an arrangement to visit London where it was planned to try some tests with the electro-encephalograph. At Soal's request I visited them, and on learning their state of mind, I made to Soal the suggestion, which he acted on, that the Jones' family should be promised any profits accruing from his writings about these experiments (p. 195). The Jones' were thus induced to make the visit to London, but evidently their suspicions were not fully allayed; for the trip was a fiasco, and both parents and boys were most unco-operative. Only chance scores were obtained in the E.E.G. experiments.

Soal verified, during a solitary visit in January 1957, that the boys were still capable of high scoring. Then in April he visited Wales, with Bowden and Mr J. E. C. Gliddon. The results were unusually erratic. Eight open-air sessions were held, spread over the ten-day visit, and each gave a chance score. Of the first four indoor sessions, the first and third gave chance scores, the second a

high score (average 11.1), and the last was of marginal significance (average 7.1). The important discovery was that even at times when he is getting chance scores under normal conditions, Glyn can get really high scores under hypnosis. The idea was born when Eira (Glyn's fourteen-year-old sister) playfully told Soal that she would hypnotize him, and then challenged him to hypnotize Glyn. Soal did so, and then did an informal test, asking Glyn to guess the card that he and Eira were looking at. The first eleven guesses were all correct. Soal then (prematurely?) decided to bring him out of the trance. Soal says he was 'absolutely certain that even had Glyn's eyes been open he could not have seen the cards'. Ieuan was not present. With the parents' permission, three further similar experiments were carried out, at two of which I was present, and in each of which Ieuan acted as Agent. It emerged that the scoring-level dwindled rapidly after Glyn had become hypnotized. Not knowing this, Soal did a trial run, at the first session before calling in witnesses. This gave 15/25. But alas, when the witnesses were present the scores were only 6, 6, 7 and 6. Next day, however, there was a striking success in the presence of Bowden and Dr Whitehead. Under hypnosis, Glyn scored 19, 24, 16, 16, 5 and 6. Before bringing him out of the trance, Soal told Glyn he would do well in another run when he woke up : and he did—15/25. This experiment was repeated next day in my presence. Glyn's scores under hypnosis were 15, 14 and 9 ; but this time the post-hypnotic suggestion failed (4/25). The first run would probably have been a very high score, if the screen had not collapsed after Glyn had guessed the first eleven cards correctly. I was satisfied that Glyn was in a hypnotic trance. As I recorded at this time, he was 'slumped in his chair, eyes closed and his speech slow and blurred'.

Soal sums up by saying : 'Glyn and Ieuan have taught us little or nothing about the nature of telepathy. . . . What this investigation does demonstrate is the all-powerful influence of an intense motivation (in this case the love of money) in maintaining the scores at a high level. . . . It is as invincible high scorers . . . that Glyn and Ieuan Jones will be remembered' (p. 235). The book contains fourteen Appendices, mostly of a statistical nature. No secondary effects were found in the data, except for a very significant tendency for the scoring rate to be higher in the second half of a sitting than in the first half. The final Appendix contains comments on the case by Dr Rhine, and by some of those who witnessed high scoring—Bateman, Fisk, Thouless, Whitehead and myself. There is one line of criticism which recurs in these comments. It is, to put it fully and bluntly, that the experimenters did

not make the best use of their opportunity ; that instead of ringing the changes to discover what cause-factors were relevant (as Soal did with such conspicuous success in his earlier investigations), the experimenters concentrated almost exclusively on maintaining high scoring, while making the conditions more and more rigorous, intimidated perhaps by critics like Dr Price. Is this criticism justified? We must remember the many difficulties the experimenters had to contend with, but still I consider, in retrospect, that better use could have been made of the peak period, in the summer of 1956 ; if, e.g., instead of multiplying independent witnesses at sittings under virtually the same conditions, more effort had been made to get results at greater distances. Two short telephone experiments were done, at other periods, without success (November 1955 and September 1956), but perhaps this method was not sufficiently explored. However, I do not accept the criticism, implicit in some of the comments in Appendix O, that the experimenters *wasted* their opportunity because they were simply proving again what we already know—that some people can exercise ESP. The unprecedented level of the scoring is itself immensely important. For one thing, it will be obvious to the least mathematically-minded that Spencer Brown's theory could not apply ; for another, it meets the familiar criticism that if ESP is genuine it ought to work oftener than once or twice in twenty-five guesses. When the scoring-level shot up in the summer of 1956, the experimenters naturally judged it vital to ensure that the conditions precluded any explanation besides ESP. Ironically, it now appears that they did not go far enough in this direction.

Before I develop this point, I want to stress that all who, like myself, were given the opportunity to witness and criticize the experiments and left our testimonials behind—and there are many distinguished names in this list—all of us share with the experimenters the responsibility for overlooking what, in retrospect may appear an obvious loop-hole. The conditions in many of the experiments certainly precluded all of the methods of deception currently recognized. We did not, however, envisage signalling by means of a supersonic or ultrasonic whistle. Such a whistle produces sounds of a very high frequency quite inaudible to most adult ears but within the hearing range of the majority of children and adolescents and many animals. Its possible use was first suggested, I believe, by Mr C. E. M. Hansel. This suggestion is briefly discussed in Appendix O by Soal (pp. 275-6) and by myself (p. 280). As our comments show, we were then both thinking in terms of a whistle concealed in Ieuan's mouth or nose, and did not, therefore, take the suggestion

very seriously. More recently, however, Mr Christopher Scott has suggested that such a whistle could have been concealed below Ieuan's clothes and operated by a rubber bulb. Now the question whether the Jones' results could be duplicated by this technique must, and doubtless will, be settled by experiment. If this proves possible, it will cast doubts on the genuineness of the Jones' results, unless this explanation can be eliminated in further tests with Glyn and Ieuan. Ultrasonic whistles are known and occasionally used in North Wales for controlling sheep-dogs and gun-dogs. I have made some enquiries and my information is that the only such whistle which is known and marketed locally is the 'Acme Silent Dog Whistle'. I don't think the Jones' results could have been achieved with this particular whistle, which is 3 inches long, and is very far from being silent to anyone's ears (it emits a loud hissing when blown). But familiarity with this make of whistle *could* have led the Jones' to enquire about and acquire a superior ultrasonic whistle.

I must conclude my review without knowing the outcome of investigation of this Whistle theory. I am prepared however to chance my arm and say that I do *not* believe that the boys *were* using a whistle, despite the fact that there are several facts which, in the light of this theory, must arouse suspicion. (1) As I pointed out in Appendix O, auditory signalling is suggested by 'the fact that the boys succeeded only when they were within earshot of each other, and that when they were in adjacent rooms their scoring usually dropped to the chance level when an intervening door was closed' (p. 279). But notice my use of 'usually'. Soal might mislead his readers when he speaks (p. 25) as if shutting the door was always followed by chance scores. This was not so. Soal was presumably referring here to the experiments in April 1956. But there were several successful sessions in November 1955 (at one of which I was present) with the door closed, and in the same rooms as the comparable experiments in April 1956. On 5 November, there was very high scoring with the door shut, and a background of noise from fireworks. However, the boys also failed (usually though not always) when instead of closing the door, one of them was shifted a foot or two sideways, so that they were no longer in alignment with the open door (see e.g. p. 61); and this change *might* have made a difference acoustically.

(2) There is the incident on 12 April, 1956, which is highly suspicious on our present hypothesis,¹ i.e. when Ieuan was unexpectedly told to put his hands over his mouth for two runs and these yielded chance scores. This fits in with the hypothesis that he had

¹ It was Mr Christopher Scott who drew my attention to the significance of this incident.

been operating a whistle manually. I am not reassured by the fact that the succeeding three runs also yielded chance scores, though Ieuan no longer had his hands over his mouth (p. 103); for he would then have been upset, on our present hypothesis. Admittedly we can't attach too much weight to a single episode, since many other changes, which should be irrelevant according to the Whistle theory, were followed by a drop in scoring. Still, it is very disquieting.

(3) The description of the boys' behaviour during the tests fits the Whistle theory. 'Glyn, when his faculty was really working well, would pause and consider before making his call. As one watched him one was often aware of his intense effort and concentration' (p. 269). (Compare this with Shackleton's and Mrs Stewart's immediate and casual responses.) Ieuan, although 'often in a sort of "trance" or "dreamy" state', 'did not show any signs of muscular relaxation. He often sat with folded arms, bolt upright, in a posture that rather suggested tension' (p. 272). The folded arms posture would be particularly convenient for manual operation of a whistle. When I was present at out-door tests Ieuan always sat thus and with knees wide apart. Soal does say: 'In a great many of our experiments Ieuan sat with his hands in full view on the table' (p. 151). But did he before 12 April? He might, after that incident, have operated a whistle by leg-movements at the indoor sessions when he sat at a table.

(4) Another feature which must arouse suspicion is the improvement in the scoring-level. I know of no other ESP subjects who have dramatically improved their scoring level after a long period of card-guessing, as the Jones' boys did in the summer of 1956. This could be easily explained on the Whistle theory, as a result of practice and improvement in technique. I cannot accept the point Soal makes on p. 254: 'Had Ieuan been signalling in these open-air experiments, it is highly probable that we should have found Glyn scoring on certain symbols rather than others.' This does not follow. What Soal says here would presumably apply to the early stages (before the signalling technique reached maximum efficiency), but not to the peak period when scores in the 20's were quite common.

(5) On the Whistle theory one would have predicted the failure of the telephone experiments, the unwillingness to co-operate in the E.E.G. experiments, and the absence of secondary effects.

Yet, despite the extent of this circumstantial evidence, I do not believe the Whistle theory, for the following reasons:

(1) *The bathing costume experiment.* The point of this was to eliminate the possibility that the boys were using miniature wire-

less sets, of about the size of a golf-ball. Bowden (EA) sat, facing Ieuan, across an improvised table—'a thin piece of wood resting on two empty . . . tins or drums' (p. 158). The region of the groin is the only place where Ieuan could have attempted to hide a whistle. The improvised table could not have concealed from Bowden much of Ieuan's thighs and legs. I find it scarcely conceivable that Bowden should have failed to notice it if Ieuan had departed from his habitual posture in the open-air tests (folded arms and legs wide apart) in such a way as to operate a whistle in his groin either by hand- or by leg-movements. Moreover, Soal reports that he searched the boys immediately after the last run. Unfortunately the record is not as explicit as one would wish. Soal says of *Glyn* that he 'felt all over' his bathing costume, including 'the insides of the boy's arms and thighs'; of *Ieuan* he says only that he 'examined his costume'. I assume that he repeated the same operations in searching Ieuan, though (presumably for stylistic reasons) he doesn't describe them in the same phrases. However, in view of the purpose of this experiment, I don't think Soal and Bowden could have been so negligent as to fail to discover a whistle with a rubber bulb concealed in Ieuan's groin.

(2) *Why did the boys resort to their crude system of cheating?* On the Whistle theory, we should be obliged to assume that it was because their whistle was lost or unserviceable. But this conflicts with the fact that the boys produced some very high scores *between* the occasions when they used their crude code (i.e. a.m. 5 November and 9 December). On the afternoon of the 5th they got 117/200 with the door shut, and the boys were certainly not then trying to use their noisy signalling. On the Whistle theory the cheating episode doesn't make sense, but it is perfectly intelligible on the assumption that the boys' results were genuine. Of course, from *our* viewpoint, it was irrational that they should try a code, since they had been getting high scores at the previous sessions in London; but it would be perfectly natural for *them* to think they could do better still by devising a code rather than relying on this mysterious and unpredictable guessing. When they made their second attempt to cheat, having made two chance scores on the same day, they did not know that their first attempt had been detected, for it was only for not sitting perfectly still that they were reprimanded after the first episode.

(3) *The unannounced change in symbols.* The success in this experiment is not conclusive, since, as Soal points out, it has been suggested that the boys *might* have prepared a system to cope with it, e.g. correlating new and old symbols by their alphabetical order. But from what I know of Ieuan, I feel sure that he was not capable

of carrying out such a plan so efficiently. (Notice that this experiment yielded an average score of 11.25, appreciably better than their normal scoring rate at that period. The earlier tests on that visit had yielded an average of 8.5 per run.) Besides, I think it extremely unlikely that the boys, after eight months' work with the same symbols, would have anticipated, and planned to cope with, a change of symbols. To my mind, the result of this experiment makes sense, on the Whistle theory, only if we suppose that Richard Jones was the maestro, who trained the boys in the whistling technique, and that he prepared them to cope with a change in symbols. This is a hypothesis which I should take very seriously, were it not that it is plainly inconsistent with the episode discussed in my last paragraph. For if Richard had been the maestro, the boys would have consulted him about a lost or broken whistle, and obviously he would not have allowed them to embark on their crude signalling, nor let them repeat it after the experimenters had complained to him about Ieuan's noisiness.

(4) *The inverse-square law.* Reeves, an expert on acoustics, comments 'perhaps the best counter to this "super-sonic" assumption is the fact that no tendency has been observed for the inverse-square law to be fulfilled'. Admittedly Reeves was envisaging whispering rather than whistling. It may be, as Scott has suggested to me, that the hypothetical whistle was clearly audible to Glyn at all ranges up to 166 feet, so that no variation of scoring-level with distance should be expected within this range. This question must obviously be settled by experiment. I am not convinced by Scott's argument however. Even at their best, the boys were getting about 50 per cent *misses*, and I would presume that *some* at least of these misses would be attributable to the whistle not carrying sufficiently 'loud and clear'. (In some successful sessions a wind was blowing from P to A, in one case a gale and then the score was 96/150 including a 24 (pp. 137-8).) The fact that runs at 60 to 83 feet gave an average score of 12.68, while runs at 120 to 166 feet gave 14.61 (p. 189) is not at all what I would expect on the Whistle theory.

(5) *The boys failures* present some difficulties for the Whistle theory. To mention only two examples, how would this theory explain (a) the failure when, according to Mrs Goldney, the boys were keen and confident of success, anxious to impress a visitor from the B.B.C., in the hope of appearing on television (p. 182); (b) the series of chance-scores during Gliddon's visit in the midst of which there was one very successful sitting (pp. 206-14)? On the Whistle theory, it would appear that the boys exercised a surprising restraint in exploiting their trick.

To sum up : I feel pretty certain that the boys were not signalling by whistle, but this is a matter of personal judgement based upon weighing probabilities and acquaintance with the people concerned, and psychical research cannot be built upon such foundations. If proof is forthcoming that the Jones' results can be duplicated by the whistle technique, my foregoing arguments would not, I think, justify a *definite* verdict in favour of ESP. Let us hope then that the boys are able and willing to produce more high scores and that the Whistle theory can be confirmed or disproved. Better that it should be confirmed than left in doubt. If these adolescents have hoodwinked so many of the experts, their place in the history of our subject will be almost as important as if they are proved innocent. I hope the question will have been settled before this review is read.

C. W. K. MUNDLE

[Since Professor Mundle wrote the above review the boys gave a demonstration in the B.B.C. 'Panorama' programme of 27 April, 1959. They made only a chance score (4 hits in 25 guesses). But, unknown to them, apparatus had been set up for the detection of any ultrasonic whistling and display on an oscilloscope. Nothing was detected either during the rehearsals or in the actual programme. In the same programme Mr and Mrs Scott demonstrated how such a whistle could be used very successfully under the same studio conditions, scoring 9 hits in 10 trials. So the question, as yet, is not settled. At least it seems certain that, in the studio, when the boys had every incentive to make a high score, no whistle was used. Ed.]

CORRESPONDENCE

SIR,—In Vol. 40, No. 699 (March 1959) of the *Journal of the S.P.R.*, Mr Denys Parsons has reviewed my paper 'Telepathy and Psychoanalysis', which appeared in the *Journal of the American S.P.R.*, Vol. 52, No. 4, October 1958.

Possibly misled by some unclarity in my paper, Mr Parsons seems to think that my meeting with the agoraphobic girl who consulted me was a matter of minutes. In fact, I talked with this girl, and got deeply interested in her problems, for about one hour. In my paper I actually wrote : 'This interview lasted for about an

hour, from 6.00 to 7.00 p.m.' I wish to make it clear that 'this' was 'my' interview, and not an interview between the girl in question and a colleague. I finally referred the patient to a colleague *for proper treatment*, because I had come to the conclusion that the case was to be treated by an analyst, and I had no time to take a new analytic case myself. But this happened when the interview was over, and the patient went to the other analyst only on the following day.

It seems to me that if *my* regular patient, Mrs A., had an agoraphobic symptom that very day at about 6.30 p.m., there is a certain time-coincidence between the two events, because at that time I was deeply engrossed in the aforesaid interview, and the agoraphobic girl had not at all 'been passed over half an hour earlier' to a colleague, as Mr Parsons believes.

Regarding the assumption by Mr Parsons that it is not surprising to see symptoms of agoraphobia, claustrophobia, etc., twice on the same day, I am ready to agree that this *can* happen in a series of interviews with new patients (most likely, in a hospital, or a clinic). However, any analyst would be surprised if a regular patient suffering from a character-neurosis, without any phobic symptom whatsoever, would suddenly develop—after more than two years of analysis—a single, transient, *unique* agoraphobic attack for no rhyme or reason, just while he, the analyst, is concerned anew with a 'pure' agoraphobic structure (a rather unfrequent thing to be seen in our epoch) at that very time of the day.

Mr Parsons has quite a right to state that 'the case histories adduced by psychiatrists . . . are usually disappointing'. In fact, it is almost impossible to give adequate expression to the ultimate feeling one gets in these cases, and I understand that no amount of details could replace such feeling. In the case commented upon by Mr Parsons, the details are certainly meagre, and this is why I have stated myself that 'I cannot prove that this was a case of telepathy'. However, I am of the opinion that accuracy in quoting psychiatrists' case histories—disappointing as they might appear—is always commendable.

EMILIO SERVADIO

Denys Parsons writes : 'First of all, I must say how very much I appreciate the courteous tone of Dr Servadio's comments on my review. As he supposes, there was some ambiguity in his paper as to who was interviewing whom on the evening in question, and I took it that his own interview with the agoraphobic girl lasted a few minutes, while the interview from 6 to 7 p.m. was between the girl and Dr. Servadio's colleague. I must apologize for this mis-

reading which meant that I was wrong in saying that there was no coincidence in time between the two events.

As to the chance probability of the coincidence, this is a matter of statistics, and I am unrepentent, since I took the precaution of sending my review to a statistician for approval before submitting it for publication.

OBITUARIES

DR HEREWARD L. CARRINGTON

Dr Hereward L. Carrington, author of many articles and books on psychical research and other subjects, lived in Hollywood, where he died on 26 December, 1958. He was born at St Helier, Jersey in 1880 and, after being educated in England, went to the United States in 1899.

He was for many years a Member of this Society, and was a Member of the Committee which reported in 1909 on Sitzings with Eusapia Palladino, his fellow members being the Hon. Everard Fielding and W. W. Baggally (*Proc.* 23, 306-560).

Besides articles on psychical research, he also wrote on fasting and dietetics, and general science, for magazines and encyclopaedias. His books included *The Physical Phenomena of Spiritualism*, *Haunted People*, co-authored with Dr Nandor Fodor; *True Ghost Stories* and *Psychic Oddities*.

He was founder-director of the American Psychical Institute of New York City, and was a life member of the International Psychical Congress, and of the American Hygienic Society.

One of his long-lived convictions was that the human soul could be photographed as it departed from the body.

MRS LYDIA ALLISON

We regret to record the death in New York on 25 March of Mrs Lydia Allison, a member of our Society from 1924 and a Corresponding Member from 1951, who had for a generation been a strong link between British and American psychical research.

Her interest in our subject arose in 1920, on the death of her husband, Dr E. W. Allison. He had, in her words, 'favoured the theory of the survival of personality after death, an attitude which I did not share.' After attending several sittings in America at which he purported to communicate, she came to England in 1923 to have sittings with Mrs Leonard and other mediums, repeating her visit for several years in succession. In this way she came to know and make firm friends with Miss Newton, my wife, and

other members of the S.P.R. The results of these English sittings (1923-7) were published in 1929 by the Boston S.P.R. in a report entitled 'Leonard and Soule Experiments in Psychological Research'.

At a later visit to England in 1929 she took a series of sittings with Mrs Leonard as 'proxy' for Mr J. F. Thomas of Detroit, and contributed a report on them to our *Proceedings*, Vol. XLII. A report by her on further proxy sittings with Mrs Leonard in 1937, 1938 was published in the *Journal* of the A.S.P.R. for October 1941. She might fairly be called an ideal sitter since, as is obvious from her reports, she established most cordial relations with Mrs Leonard, and her Control, Feda, could encourage the process of communication without giving information away, could tactfully check the tendency of even the best Controls to ramble, and was a shrewd judge of the evidential value of the various points raised in a sitting.

In 1925 a split in the American S.P.R. on questions of policy, including the line to be taken with regard to the Margery mediumship, resulted in the formation of the Boston S.P.R., of which Mrs Allison became a Council Member together with other distinguished psychical researchers such as Elwood Worcester, McDougall, Gardner Murphy and Walter Prince. The Boston Society, which had during its life a fine record of publishing important studies, continued until 1941 when changed circumstances made possible a reunion with the Society in New York.

The news of the reunion was conveyed to our Society by Mrs Allison (*S.P.R. Journal* for June-July 1941), and I have always understood that her tact and her desire that there should be no needless divisions between persons committed to studies as important as psychical research contributed very largely to this happy result. On the amalgamation taking effect she became a member of the Board of Trustees. She was appointed Secretary in 1944, and had for several years before her death been Chairman of the Publications Committee. Friendly, co-operative, shrewd, candid, discreet, she was a person to whom one could write and speak freely on matters alike of research and administration. That would, I am sure, be the testimony of all the officers of our Society who were brought into touch with her.

Mrs Allison's knowledge of psychical affairs, historical and contemporary, was immense. This particularly struck me when enjoying her hospitality in New York in 1950, and again during discussions with her at the Utrecht and Cambridge Conferences in 1953 and 1955. She read an interesting paper on American poltergeists at Cambridge, but she was beginning to show signs of being tired, and from then on her health declined, without how-

ever diminishing in any degree her interest in psychical research or her desire to promote it to the full limit of her strength.

W. H. SALTER

When I first met Mrs Allison in the 1920's I soon came to the conclusion that she had remarkable qualities and would make an admirable psychical researcher. Although a recent bereavement had caused her a profound emotional shock, she never wavered in her determination not to let her feelings influence her too deeply in her search for evidence for survival. She knew a great deal that was going on and I found her advice invaluable when making contacts with persons whom she knew but whom I was meeting for the first time. I soon came to recognize the value of that advice and the wisdom and charity which always animated her.

It was not only in the mental phenomena that her interests lay. Recognizing the difficulties under which the American S.P.R. was at that time labouring, she saw that some additional accommodation was at that time necessary for the investigation of physical phenomena, and it was through her generosity and help that a small room was discovered in which such work could be carried on. The American Society owes her a great debt for the work that she did for it and psychical research loses an outstanding personality of exceptional acumen and intellectual integrity.

E. J. DINGWALL

I met Mrs Allison once only during the Cambridge Conference on Spontaneous Cases in 1955. Before that she had been no more than a name to me, stimulating, but out of time. Her impact as a person was vivid. There was the zest of the true researcher about her, and it was clear that she would stand no nonsense from her frail and aging body. In fact, of all attending the Conference she seemed one of the most receptive, open-minded and youngest in spirit. I have often thought of her since to encourage myself at times when my own energy and enthusiasm were flagging.

ROSALIND HEYWOOD

We regret to have to record the death, on 22 April, 1959, of Mrs W. H. Salter. She joined the Society in 1905, and, after valuable service as a Member of Council for many years, was elected Vice-President in 1953. An obituary notice will be published in the September Journal.

EXCERPTA

From 'D. H. Lawrence : a composite biography', edited by Edward Nehls, (*University of Wisconsin Press*, 1959, Vol. III, pp. 480-1). From a letter written by Mrs Jessie Chambers Wood to Helen Corke in 1933.

... STRANGELY enough, the record will extend just beyond his death. . . . As I have said the fact of D. H. L.'s suffering is the dominant fact in his life for me, and it was only after the publication of *The Plumed Serpent* that I realized he was a tortured spirit. As you know, I returned his last letter in 1913, and since then no word has passed between us, and I never heard news of him ; his name was never mentioned to me. I did not know he was ill ; the letter he sent to David was never shown to me until weeks after his death, so that whatever knowledge I had of him came through other channels than those of ordinary communication. For some eighteen months or so before his death I felt acutely drawn to him at times and wondered intensely how some kind of communication that seemed so urgently needed was to be established. It seemed to be not just a matter of writing a letter—something else, something different was needed. The feeling that some drawing together was imminent scarcely ever left me. Once quite suddenly, as though he had spoken, the words came into my mind, 'We are still on the same planet.' There were other things too of a like nature. Please remember I had no idea D.H.L. was ill. On the morning of the day he died, he suddenly said to me, as distinctly as if he had been in the room with me : 'Can you remember only the pain and none of the joy?' and his voice was so full of reproach that I made haste to assure him that I *did* remember the joy. Then later on in a strange confused way he said—'What has it all been about?'

The next morning I was busy with housework when suddenly the room was filled with his presence and for a moment I saw him just as I had known him in early days, with the little cap on the back of his head. That momentary presence was so full of joy that I simply concluded it was an earnest of a real meeting in the near future. I remember saying to myself, 'Now I *know* we're going to meet.'

The following day his death was announced in the paper and was a terrible shock to me. I give you this for what it is worth . . . smile it away if you will, it doesn't matter ; the experience was just as real as the fact I am now holding a pen. I don't think it was self-suggestion because I didn't know he was ill ; I was full of anxiety on his behalf, but I judged his trouble to be of the soul.

(Laurence died at Vence, near Nice, at 9.00 p.m. on 2 March,

1930. Jessie Chambers was a young schoolmistress whom it is likely Lawrence would have married but for the opposition of his mother. She married a schoolmaster, J. R. Wood, in 1915 and died in 1944. 'David' was Jessie's brother.)

NOTICES

A REVISED edition of the pamphlet *Psychical Research : A Selective Guide to Publications in English*, recommended by the Council of the Society, has now been published and copies may be obtained from the Secretary (1s. 2d. post-free). The original edition was published in 1949.

ADDITIONS TO THE LIBRARY

The Nature of Experience by Sir Russell Brain. Published by the Oxford University Press, 1959. Price 8s. 6d.

The Easter Enigma by Rev. Michael C. Perry. Published by Faber, 1959. Price 21s.

Natural Selection and Heredity by P. M. Sheppard. Published by Hutchinson, 1958. Price 18s.

Brain Mechanisms and Consciousness. A Symposium organized by The Council for International Organizations of Medical Sciences. Published by Blackwells, Oxford, 1956. Price 42s.

The Mind Readers by S. G. Soal and H. T. Bowden. Published by Faber, 1959. Price 30s.

Die Phantome von Kopenhagen by Dr Hans Gerloff. Published by Verlag Welt und Wissen. Bidingen-Gettenbach. 1958.

The Sixth Sense by Rosalind Heywood. Published by Chatto & Windus, 1959. Price 21s.

Memory : Facts and Fallacies by Ian Hunter. Published by Pelican Books, 1957. Price 3s. 6d.